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THE INFLUENCE OF THE LEVEL OF PERFORMANCE IN ONE TASK ON THE LEVEL OF ASPIRATION IN ANOTHER

BY JEROME D. FRANK

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The level of aspiration may be defined as an individual's goal or expectation in regard to the goodness of his own future performance in a given task. It was first extensively investigated by Hoppe (**6**), while studies of certain of its aspects have been reported by Fajans (**2**), Hausmann (**5**), and the present writer (**3, 4**). All of these studies are concerned with the behavior of the level of aspiration in a task as a function of performance in the same task; the present investigation yields preliminary data concerning the behavior of the level of aspiration in one task as a function of performance in another task.[1]

Five tasks were used, each involving the repetition of some simple standardized performance. The goodness of the performance was given by the number of seconds required to complete it. After each repetition the *S* was told the number of seconds he had required (his *level of performance*), and asked to state the number of seconds he would require to complete the next repetition (his *level of aspiration*). A brief description of the tasks follows.

[1] The writer wishes to express his gratitude to Dr. J. G. Beebe-Center of Harvard University for guidance and advice in the carrying out of this investigation.

1. *Printing with Small Letters:* The materials used were a set of wooden printing blocks with rubber faces, lower case only, a stick of wood with a metal runner to be used as a guide in printing, large sheets of paper on which the printing was done, and six typewritten cards containing the material to be printed. All the cards contained the same number of words and letters and were standardized as to difficulty. The *S* was told to work as rapidly as possible.

2. *Printing with Capital Letters:* This task differed from that above only in the use of upper case letters and a different set of cards to be printed. These cards were uniform with those of the previous task as regards difficulty.

3. *Designs:* The materials of this task were a hexagonal peg board containing thirty-two holes, thirty-two colored pegs, of which eight were green, eight red, eight blue, and eight orange, and six cards, each containing a diagram of the board with a design marked on it in colors corresponding to the colors of the pegs. The designs were of approximately equal difficulty. The *S* was required to copy the designs by placing the pegs in the proper holes as rapidly as possible. The pegs were placed in holes around the rim of the board when not in use, and returned there before each repetition.

4. *Logical Relations:* The materials for this task consisted of eleven mimeographed sheets, each containing eleven triads of words. Each triad consisted of a related pair and a third word, *e.g.*, 'good—better, bad—.' The *S* was required to find, as rapidly as possible, a fourth word which bore the same relation to the third as the second did to the first. The triads were composed by combining words from the lists given by Woodworth and Wells (8), care being taken to avoid ambiguous pairs. Each sheet contained from five to seven different types of relations, and all were approximately equal in difficulty.

5. *Spatial Relations:* This task consisted of strips of paper containing sections of the Kelley Manipulation of Spatial Relationships Speed Test (7). Each strip contained two rows, and an attempt was made to equate the strips in difficulty. The *S* was required to perform the necessary operations on each strip as rapidly as he could.

In all these tasks the *S* was timed by means of a Telechron table clock, in plain sight of both *S* and *E*. The situation was kept as free as possible—the instructions were imparted conversationally and the *S* was allowed to take all the time he wished between trials.

The tasks were grouped into the following three pairs:

1. Printing with Small Letters and Printing with Capital Letters—two motor tasks, almost identical.

2. Printing and Designs—two motor tasks differing in all respects except the scale of difficulty.

3. Spatial Relations and Logical Relations—two intellectual tasks differing in all respects except the scale of difficulty.

Each pair of tasks was given to a group of eight *S*s, all undergraduates or graduates in Harvard University, concentrating in Psychology.

The experimental procedure for all three pairs of tasks was the same. The *S*s were taken singly in three sessions about a week apart, in each of which both tasks of a pair were given. The tasks were given in the same order throughout for each *S*, but half of each group received one task first, the other half the other. In the first session each task was carefully explained to the *S* and he was allowed one trial before he was required to state his level of aspiration. This was followed by ten repetitions, for each of which the level of performance and level of aspiration were recorded as

described above. There was a pause of a few minutes between the two tasks. In the second and third sessions the procedure was *ostensibly* the same as in the first, except that (1) there were only five repetitions of each task, (2) the *S* was made to state his first level of aspiration in each task before he tried it. In order to give him something on which to base his judgment, he was told his last or best level of performance on the same task in the previous session. Whether the last or best was given him was left to his choice.[2] *Actually* the second and third sessions differed from the first also in that the *E*, without the *S*'s knowledge, falsified the performance times on the *first* task of each pair. In one session the *S* was given performance times *shorter* than those he actually made (his level of performance was *raised*), while in the other the *S* was told that his performance times were *longer* than was actually the case (his level of performance was *lowered*).[3] This task will henceforth be termed the *easy and hard* task. It will be called the *easy* task when the performance level was raised, and the *hard* task when the performance level was lowered. The *S*'s level of performance as known to himself was thus higher (usually by about ten seconds) in the easy than in the hard task.

After five repetitions of the easy or hard task, there was a short pause, and then the *S* was asked to state his level of aspiration in the second task, also before trying it. There were then five repetitions of the second task, on each of which the *S*'s level of aspiration was obtained. In both sessions the *S* was told the performance levels he actually made in this task, henceforth termed the *normal* task. One half of the *S*s of each group received the easy task in the second session and the hard task in the third, the other half received the hard task in the second session and the easy task in the third. For half the *S*s one task of a pair was kept normal, for the other half the other. For each *S* the same task was made easy and hard, and the other remained normal, throughout.

The effect of the shift in the level of performance in the easy and hard task on the height of the level of aspiration in the normal task can be determined by a comparison of the heights of the level of aspiration in the normal task in the second and third sessions. For reasons which will become apparent it is necessary to divide the results into two groups, in one of which the first level of aspiration in the normal task will be considered, in the other the average of the remaining levels of aspiration in the normal task.

Table I presents the average of the first levels of aspiration in the normal task after the easy and after the hard task.

[2] It was found in preliminary experiments that some *S*s remembered their levels of performance in the previous session. These *S*s based their first levels of aspiration on the last or best level of performance as they saw fit, and regardless of the instructions of the *E*. This possible source of confusion in the results seemed to be best eliminated by first asking the *S* on which level of performance he wished to base his first level of aspiration.

[3] This deception could be carried out without difficulty since the *S*s paid no attention to the clock, and the fact that it was in plain sight the whole time allayed their suspicions completely.

TABLE I

HEIGHT OF FIRST LEVEL OF ASPIRATION IN THE NORMAL TASK

Tasks	1 Average Height after Easy Task	2 Average Height after Hard Task	3 1 minus 2	4 % of Cases Positive
Printing-Printing	80.25	76.75	+ 3.50	87.5
Printing-Designs	71.75	69.75	+ 2.00	62.5
Spatial Rel.-Logical Rel.	83.38	80.12	+ 3.26	62.5

The pairs of tasks are presented in the left-hand column. In Column 1 is given the average of the first levels of aspiration in the normal task after the easy task, for each of the three pairs of tasks. These averages are expressed in terms of 120 minus the actual number of seconds given by the *S* as his level of aspiration, so that a higher level of aspiration is represented by a larger number. In Column 2 the corresponding data for the first level of aspiration in the normal task after the hard task are given. Column 3 contains the difference between Column 1 and Column 2 in terms of seconds. A positive difference indicates that the average of the first levels of aspiration in the normal task was higher after the easy task than after the hard task.[4] In order to give some idea of the validity of these differences, the percentage of individuals in each group who showed a shift in the same direction as the shift of the average is given. For each pair of tasks, this percentage is based on eight individuals.

These results seem to indicate that the performance level in the hard and easy tasks does exert some effect on the height of the first level of aspiration in the normal task. However, these results contain an extraneous factor which it would be well to eliminate if possible. The first level of aspiration in the normal task depends not only on the performance level in the easy and hard task but also on the performance level in the normal task in the previous session. That is, the *S* consciously based his first level of aspiration in the normal task in each session on his last or best performance level in that task

[4] It should again be mentioned that the task which was normal for four of the *S*s in each group was easy and hard for the remaining four, so that the results obtained are independent of which task was easy and hard and which normal.

the previous week. Since the performance level rises with practice, this alone would be sufficient to make the first level of aspiration in the normal task higher in the third than in the second session. This means that for the four *S*s in each group who had the easy task in the second session and the hard task in the third session, the practice and transfer effects oppose each other, while with the remaining four they work in the same direction. As a result the absolute height of the first level of aspiration in the normal task is not a good measure of the extent of the transfer effect.

The effect of the level of performance in the normal task in the previous session can be eliminated by using as the criterion of the transfer effect the difference between the first level of aspiration in the normal task and the last or best level of performance in the same task in the previous session. (See p. 161 above.) If this difference tends to be greater after the easy task, such a tendency can be due presumably only to the transfer effect.

The averages of the differences between the first levels of aspiration in the normal task and the last or best levels of performance in the same task in the previous session are given in Table II.

TABLE II

FIRST LEVEL OF ASPIRATION IN THE NORMAL TASK MINUS LAST OR BEST LEVEL OF PERFORMANCE IN THE NORMAL TASK IN THE PREVIOUS SESSION

Tasks	1 Average Difference after Easy Task	2 Average Difference after Hard Task	3 1 minus 2	4 % of Cases Positive
Printing-Printing	− 1.25	− 7.00	+ 5.75	75
Printing-Designs	− 2.12	− 3.50	+ 1.38	50
Spatial Rel.-Logical Rel.	+ 0.87	− 2.75	+ 3.62	87.5

As in Table I, the tasks are given to the left, Column 1 contains the average of the differences after the easy task, Column 2 the average of the differences after the hard task, Column 3 the differences between Columns 1 and 2, and Column 4 the percentage of cases showing a shift in the direction of the averages. The data in Columns 1, 2, and 3 are in terms of seconds.

The elimination of the effect of the level of performance in the normal task has made the results of the third group more clearly positive, and has decreased the reliability of the results of the second group, but has left the order of the three groups unchanged. The conclusion may be drawn that there is a definite effect of the height of the performance level in one task on the height of the first level of aspiration in another task. Furthermore, this effect has been demonstrated in both motor and intellectual tasks. The striking difference between the results of Printing-Printing and Printing-Designs leads to the tentative conclusion that the effect is to some extent proportional to the degree of similarity of the tasks.

The effect of the level of performance in the easy and hard task on the average height of the remaining four levels of aspiration in the normal task is given in Table III. This

TABLE III

AVERAGE HEIGHTS OF THE LEVELS OF ASPIRATION ON TRIALS 2 TO 5 IN THE NORMAL TASK

Tasks	1 Average of the Average Heights after Easy Task	2 Average of the Average Heights after Hard Task	3 1 minus 2	4 % of Cases Positive
Printing-Printing	84.69	84.46	+ 0.23	50
Printing-Designs	77.88	75.54	+ 2.34	62.5
Logical Rel.-Spatial Rel.	88.62	86.96	+ 1.66	62.5

table contains the averages of the average heights of the levels of aspiration on trials two to five in the normal task after the easy and after the hard task.

The arrangement of this table is identical with that of Table I, and the data are expressed in terms of the same units.

In none of these three groups would it be safe to assert positively that the levels of performance in the easy and hard task affect the average level of aspiration in the normal task. Nevertheless, it is significant that the results are always positive, however slightly.

As before, these results are marred by the indirect inclusion of the level of performance in the normal task, which would tend to make the average level of aspiration in the third session

higher than that in the second, regardless of the easy and hard task. This influence has been eliminated in Table IV. In

TABLE IV

AVERAGE LEVEL OF ASPIRATION ON TRIALS 2 TO 5 MINUS MEDIAN LEVEL OF PERFORMANCE ON TRIALS 1 TO 4 IN THE NORMAL TASK

Tasks	1 Average Difference after Easy Task	2 Average Difference after Hard Task	3 1 minus 2	4 % of Cases Positive
Printing-Printing	+ 0.94	+ 0.28	+ 0.66	50
Printing-Designs	+ 3.88	+ 3.53	+ 0.35	75
Logical Rel.-Spatial Rel.	+ 3.50	+ 2.21	+ 1.29	62.5

this table the results have been calculated by subtracting the median level of performance on trials one to four from the average level of aspiration of trials two to five, and averaging these differences for the eight *Ss.*[5] The arrangement of Table IV is identical with that of Table II and the data are expressed in the same terms.

Again the results, though not markedly positive, are positive throughout. The conclusion may be risked that there is some slight effect of the level of performance in one task on the average height of the level of aspiration, both absolutely and relative to the height of the level of performance, in the other. To the extent to which there is such an effect, it can be demonstrated in both motor and intellectual tasks. Furthermore, over the range of similarities represented by the two pairs of motor tasks there is no evidence that the effect bears any relation to the degree of similarity of the tasks. These results will be discussed in more detail below.

A final statistical result of interest is that the effect of the level of performance in the easy and hard task on the first level of aspiration in the normal task shows no relationship to its effect on the average of the remaining levels of aspiration in the normal task. If the shifts in the first level of aspiration

[5] The median level of performance was used in order to minimize the effect of extreme values, which were regularly regarded as 'flukes' by the *S*, and as such usually left the level of aspiration unaffected. The last level of performance is omitted because it has no corresponding level of aspiration. Compare the present writer's earlier study (3).

and the shifts in the average of the remaining levels of aspiration in the normal task (both relative to the level of performance—the data on which Columns 3 of Tables II and IV are based) are correlated by the rank order method, rho equals 0.02, with a P.E. of 0.14.[6] In other words, there is no necessary relation between the effect of the level of performance in one task on the first level of aspiration, and its effect on the succeeding levels of aspiration in another task.

These results are significant with reference to a general theory of the behavior of the level of aspiration outlined elsewhere (**6, 4**). According to this hypothesis, the level of aspiration is governed in varying proportions by two main influences. The first of these is the need to make the level of aspiration approximate the level of performance. It is assumed to be always present to some degree, and its effect is to keep the level of aspiration close to the level of performance. Its influence is masked in some cases by the need to protect the *ego-level* when this is involved in the task. The ego-level is conceived to be a complex psychical system representing the sum total of an individual's goals and aspirations. It has the fundamental property that it tends to be kept high at all costs. The ego-level may be said to be involved in the task when the *S* regards the height of his level of performance (and sometimes the height of his level of aspiration as well) as in some sense a measure of his own worth, either in his own eyes or the eyes of others. It is this involvement of the ego-level which gives rise to experiences of success when the level of performance is above the level of aspiration, and of failure when the level of performance falls below the level of aspiration (**6**). The involvement of the ego-level produces a tendency in the *S* to set his level of aspiration consistently either higher or lower than his level of performance warrants. Both types of behavior may be regarded as attempts to protect the ego-level, the former by ignoring failures, the latter by avoiding their occurrence. In short, the level of aspiration is at one extreme an objective estimate of the probable level of future perform-

[6] Correlation of shifts in the *absolute* values of the first and average levels of aspiration in the normal task is inadmissible, since the effect of practice is common to both sets of data.

ance based on the level of past performance, and at the other a means of protecting the ego-level, more or less regardless of the level of performance (4).

Applying this hypothesis to the results, it is apparent that even when the level of aspiration represents solely an objective estimate, the results obtained with the first level of aspiration in the normal task are to be expected. In the two last sessions the *S* must state his first level of aspiration in the normal task before he has any level of performance in that task to guide him. In so far as his level of aspiration is an objective estimate, he will tend to base it partly on his levels of performance in the normal task in the previous session, and partly on his levels of performance in the easy and hard task in the same session. The extent of the influence of the levels of performance in the easy and hard task will depend on the degree to which the *S* regards the two tasks as essentially the same. This in turn will largely depend on the extent to which the two tasks are objectively similar. To take a concrete example, the *S* will probably consider one of the printing tasks simply as a continuation of the other with trifling changes in technique, while Designs and Printing will usually be regarded as distinctly different tasks. As a result, the first level of aspiration in one printing task would be influenced more by the immediately preceding levels of performance in the other printing task than it would be by the immediately preceding levels of performance in Designs. In short, when the level of aspiration represents an objective estimate, we should expect the first level of aspiration in the normal task to be higher after the easy than after the hard task, the extent of the effect depending on the degree of similarity of the tasks. This is, in fact, the general trend of the results. Most of the exceptions are due to the involvement of the ego-level in the first level of aspiration, as will be discussed more fully below.

In so far as the level of aspiration in the normal task is solely an objective estimate, it should cease to be influenced by the levels of performance in the easy and hard task as soon as the *S* has a level of performance in the normal task on which to base his judgment. The effect of the levels of performance in

the easy and hard task on the average height of all levels of aspiration except the first in the normal task can only be the result of some change in a deeper structure of the personality which influences the levels of aspiration in both tasks. It is postulated that this structure is the ego-level. As pointed out above, when the ego-level is involved in a task, the level of performance takes on a dynamic significance—it is no longer simply above or below the level of aspiration, it is a success or a failure (**6**). The central problem then becomes the determination of the effect of success and failure in one task on the average height of the levels of aspiration in another. A major difficulty in the application of the results to this problem is the fact that the effect of success and failure on the level of aspiration in a single task has not yet been fully analyzed. The statistical analysis of the results made the tacit assumption that failures in the hard task always lowered the level of aspiration in that task. Though this is usually true, it is not always so, and at least one of the exceptions shows a clear transfer effect. The effect of failure on this *S* was to make him *raise* his level of aspiration and try all the harder. In the hard task, despite an average level of performance of thirty-five seconds, he finally pegged his level of aspiration at twenty seconds, over eight seconds *higher* than his average level of aspiration in the easy task. In the pause, he said, "I've got to get twenty seconds in *something!*" He then kept his level of aspiration at twenty seconds throughout the normal task, despite continuous failure. This was five seconds higher than the average of his levels of aspiration in the normal task after the easy task. In short, the effect of failure in the hard task was to produce a fixing of the level of aspiration in *both* tasks. Yet such are the exigencies of the statistical method that this *S* appears as a negative case as regards both his first and average levels of aspiration in the normal task. Certain other apparently negative results also appear to be positive in the light of these considerations. A fully satisfactory treatment of the effect of success and failure in one task on the average height of the level of aspiration in another must thus await a

further elucidation of the effects of success and failure on the level of aspiration in a single task.

Some evidence that an effect of the level of performance in one task on the average height of the level of aspiration in another depends on the involvement of the ego-level in both tasks is afforded by the following relationship. There are six *Ss* who show a shift of greater than 1.5 in both the absolute and relative heights of the average level of aspiration in the normal task, in the same direction as the shift of the level of aspiration in the easy and hard task. To these, two *Ss* may be added (the results of one of whom are discussed above) whose remarks during the experiment clearly indicate a transfer effect which, however, fails to appear in the relative height of the average level of aspiration in the normal task because of wide irregularities in the level of performance. There are thus eight *Ss* for whom a transfer effect on the average level of aspiration in the normal task seems clearly indicated. Of these eight, six exhibit that acute awareness of the *E* which seems to indicate a situation of 'social pressure,' and which has elsewhere been suggested as a reliable criterion of the involvement of the ego-level (**4**). There is only one *S* who shows social pressure without a definite transfer effect; while only 2 *Ss* show a probable transfer effect without demonstrating marked social pressure. In short, the presence of social pressure coincides to a remarkable degree with the presence of a demonstrable effect of the level of performance in one task on the average level of aspiration in another, lending support to the assumption that the involvement of the ego-level is a necessary condition for this effect. This assumption is consistent with the apparent independence of this transfer effect from the degree of similarity of the tasks. The involvement of the ego-level in both tasks will presumably depend much more on personal and general situational factors than on the particular properties of the tasks. Tasks with widely different properties may be brought into close functional relationship through the fact that the *S* regards his level of performance in both of them as expressions of his own worth.

In conclusion, it should be pointed out that this hypothesis

is consistent with the utter lack of relationship between the behavior of the first level of aspiration and that of the later ones in the normal task (see page 164 above). The two groups of results will tend to diverge as often as they coincide both when the ego-level is involved and when it is not. When the ego-level is not involved, the levels of performance in the easy and hard task will affect only the first level of aspiration in the normal task (see page 165 above). Whether this effect coincides with the behavior of the later levels of aspiration (which depends solely on the levels of performance in the normal task) is purely a matter of chance. When the ego-level is involved in the task, there are good grounds for assuming that it tends to be less involved in the *first* level of aspiration of a task than in the later ones.[7] Therefore the first level of aspiration in the normal task will be more of an objective estimate than the later ones, so that here, too, the two groups of results are relatively independent of each other.

To sum up, all the results obtained seem to be adequately explained by the hypothesis that the level of aspiration varies between a purely objective estimate of future performance on the basis of past performance, and a means of protecting the ego-level. Changes in the level of performance in one task affect the first level of aspiration in another even when the level of aspiration is solely an objective estimate, but can affect the remaining levels of aspiration only when the ego-level is involved in both tasks.

Summary

1. Changes in the level of performance in one task affect the height of the first level of aspiration in another. The extent of this effect depends on the degree to which the two tasks are objectively similar.

2. Changes in the level of performance in one task affect the average height of the remaining levels of aspiration in

[7] Indirect evidence for such a hypothesis is found in Hoppe's observation that the first level of aspiration in a task tends to be lower than the succeeding ones (6), and in the discovery of Birenbaum that two "psychical systems" which have become united through frequent repetitions tend to separate temporarily in the first repetition after a pause (1).

another in some cases. This effect seeks to be independent of the degree of objective similarity of the tasks, and to depend rather on individual factors.

3. There is no evidence that these two types of effect are related.

4. The results obtained are shown to accord well with the hypothesis that the level of aspiration represents on the one hand an objective estimate of the future levels of performance on the basis of past levels of performance, and on the other a means of protecting the ego-level when this is involved in the task.

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SOME CAUSAL FACTORS IN MENTAL BLOCKING

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This article describes one of a series of investigations whose purpose is to discover the nature of mental fatigue and the principles governing it. Two previous studies have appeared which throw light on the problem of mental fatigue and explain the failure of students of the work curve to obtain large decrements in the objective records of continuous mental effort. One of these (1) clarifies the nature of difficulty in mental tasks, and shows that when these factors of difficulty are present, the objective decrement in mental work is comparable to that obtained in physical effort. The two chief factors are homogeneity and competition. The more homogeneous a task is, the steeper will be the decrement developing in it, unless the elements in the task conflict with one another. In the latter case, where competition is present, the principle is reversed—the greater the number of elements, the steeper the decrement. The other study (2) reports the discovery of the principle of mental blocking. This name is given to the tendency shown by practically all persons, in carrying on continuous mental work, to show periodic gaps or pauses in their responses, which they are unable to prevent, no matter how hard they try. These breaks were found to occur about three times a minute on the average, and to have a duration of from 2 to 6 average response times; though practice decreases and fatigue increases their length and frequency, and individuals differ from one another widely. I have associated these blocks with the biological phenomenon of refractory phase, on the assumption that they form enforced resting periods at frequent intervals and thereby allow the more delicate mechanisms involved in mental reactions to recuper-

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ate, before impairment can occur. There is no question but that the decrements in continuous mental tasks would be steeper if it were not for these frequent rests.

If the foregoing assumption is correct, that these blocks or pauses form a cushion against fatigue, then there should be more and longer blocks in those tasks which are more fatiguing. That, according to the principles previously formulated, means either tasks which are homogeneous, or those in which the elements compete with one another and cause conflict. The question which the present study attempts to answer, then, is this: Does the frequency and duration of the blocks resulting from continuous mental work vary directly with the homogeneity and amount of competition present in the task? Of course it would be incorrect to assume that the mere presence of more and longer blocks in such tasks is proof of their function as rest periods, since homogeneous and competitive tasks do nevertheless show greater decrements. But at least there is a possibility that the decrement might be even larger except for the more frequent rests.

To answer this question two main experiments were carried out. In Experiment I, the factor of competition was investigated. Sixteen subjects were given the task of continuously naming colors arranged on large charts. Three different types of chart were used, in order to give three different conditions, each with a different degree of competition. In Condition I, the chart contained five different colors, arranged in scrambled order, red, blue, yellow, green and black. The squares of color, each one square centimeter in size, were in ten horizontal rows, ten to the row. When a chart had been read through once, it was turned 90 degrees to the right and read through again, so that four different orders were obtained. In Condition II, the chart contained three different colors in scrambled order, and in Condition III, two different colors were used. The responses were automatically recorded on kymograph paper, and later analyzed to determine the number of responses per minute, and the decrement in successive minutes. The subjects worked for ten successive minutes. The number of blocks and average

size of blocks for each successive minute were also determined. The experiment was carried through three cycles, and the order of taking conditions was varied systematically for each cycle and for each individual, to equalize practice.

When the results for the three cycles are averaged they show the following things:

The amount of decrement in number of responses per minute varies with the condition, being greatest for the most competitive and smallest for the least competitive condition. In Condition I, there is a drop from 106 responses to 96 responses per minute, or about 10 percent. In Condition II, from 133.5 to 126.5, or about 5.3 percent. In Condition III, from 150.6 to 145.6, or about 3.3 percent. The results for the third cycle alone, which is the most stable, are given graphically in Fig. 1. The curves are plotted on a percentage basis, calling the initial level 100 percent, so that all three curves have the same origin. It is necessary to ignore the large end spurt in comparing the curves. The decrement is not very large, even in the most competitive condition. Therefore, if the differences between the frequency and size of blocks in the different conditions turn out to be large, we can conclude that the blocks have compensated in some manner for fatigue and have kept the decrements small, in spite of excessive competition.

The amount of blocking does vary with the condition, being greatest in the most competitive condition. In Condition I, the average number per minute was 3.55, whereas it was 3.3 in Condition II, and 3.0 in Condition III. This relation is shown graphically in Fig. 1. However, a more important question is whether there was an increase in number of blocks per minute during the work period, and whether this increase was greater for one condition than another. By comparing the blocks per minute for the first half versus the last half of each work period, for each condition, it is possible to answer this question. In Condition I, there was an increase from 3.4 to 3.7 blocks per minute; in Condition II, there was no increase; and in Condition III, the increase was from 2.9 to 3.1.

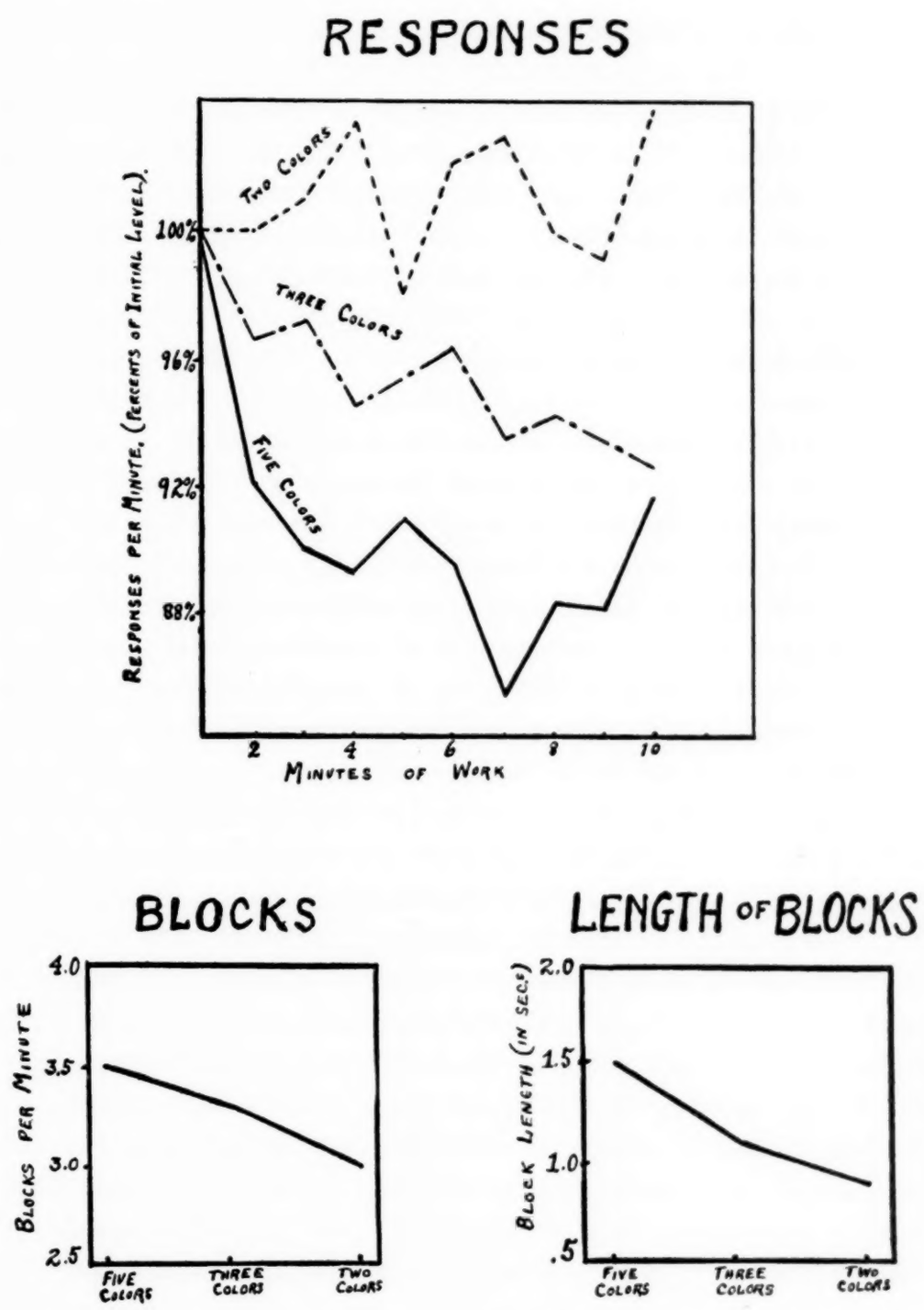

FIG. 1. Competition and blocking. The first graph shows the greatest decrement in the work curve for the most heterogeneous condition. The second and third show the greater frequency and length of blocks for this condition.

The length of the blocks varies with the condition also, the longest blocks occurring in the most competitive condition. In Condition I, the average length of a block was 1.45 seconds;

in Condition II, it was 1.1 seconds, and in Condition III, 0.95 seconds. This is shown graphically in Fig. 1.

There is an increase in the length of the blocks during the work period, and this increase varies with the condition, being greatest for the most competitive condition. In Condition I, the length increased from 1.3 seconds in the first half of the work period to 1.6 in the last half of the work period. In Condition II, this increase was from 1.0 to 1.2; and in Condition III, from 0.9 to 1.0 roughly.

These facts, taken in conjunction, point strongly to a compensatory function of the blocks in staving off fatigue, especially since the percentage increase in blocking for the more fatiguing conditions is so much greater than the percentage increase in speed decrement for those same conditions.

Experiment II was concerned with the factor of homogeneity as it affects the amount of blocking. In the experiment of Robinson and Bills (1), it was found that the continuous writing of letter sequences produced a highly homogeneous type of work, in which the amount of homogeneity could be systematically varied by varying the number of letters in a sequence. Thus the sequence abababababab, etc., is a more homogeneous one than the sequence abcdefabcdefabcdef, etc. This task was therefore selected for the present study. Unlike the task of naming colors in scrambled order, this letter-naming response does not produce conflict or competition in proportion to the number of distinct elements, since the letters are always written in the same order. Therefore, the factor of competition is removed, and the only important variable is the degree of homogeneity of the task. In order to record the written responses kymographically, so that they could be analyzed later for blocks and time elapsing between responses, it was necessary to use a recording stylus controlling a signal marker. The device used proved to be just clumsy enough in operation so that there was a delay with each response, and the subject's mental reactions went ahead of his manual responses. The result was that, instead of a decrement, there was an increment in speed during the work period, and almost no blocks occurred. The results are omitted.

One of the subjects of the experiment, Miss Jean Rosenbluth, became interested enough to repeat the experiment under my direction. She carried out the study under much more favorable conditions, with an increased number of subjects, and a method of recording the responses which eliminated the delay in manual reaction and made it possible for both fatigue and blocks to appear. The following results are therefore taken from her data.

Twenty-four subjects were used. They were seated comfortably at a table and wrote the letter sequences with ordinary pencil on special paper, writing two letters at a time, *i.e.*, ab ab ab ab, and lifting the pencil off the page after each pair. The experimenter observed them from behind a screen and recorded the time of each response kymographically by pressing an electric key connected with a signal marker. Two conditions were employed, Condition I using a six-letter sequence and Condition II using a two-letter sequence. In order to be sure that the difference did not depend on the particular letters chosen there were three different versions of the two-letter sequence used, one with ababab, one with cdcdcd, and one with efefef. The experiment was carried through three cycles, and the different subjects went through the conditions in scrambled order to equalize practice effects. The length of a single work period was ten minutes.

The study brings out the following facts:

The amount of decrement in speed varies with the condition, being greater in the more homogeneous one. In Condition I, there was an actual gain from 154 to 156 responses per minute; in Condition II, there was a drop from 152 to 142 responses per minute. This is shown graphically in Fig. 2, where the work curves for the two conditions are compared. This merely corroborates the study of Robinson and Bills (1), in demonstrating the relation between fatigue and homogeneity.

The average number of blocks per minute is greater for the more homogeneous condition. In Condition I, there is an average of 1.4 blocks per minute, and in Condition II, 2.1 per minute.

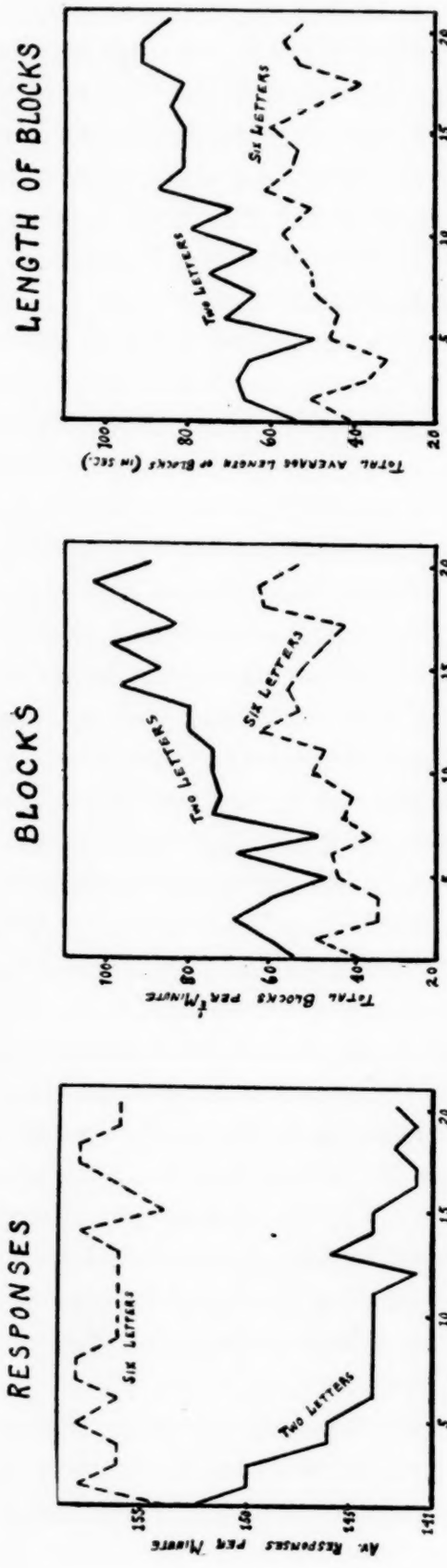

FIG. 2. Homogeneity and blocking. The first graph shows the greater decrement in the work curve for the more homogeneous condition. The second and third show the greater increase in frequency and length of blocks in this condition.

The homogeneous condition produces a greater increase in block frequency during the work period. In Condition I, the blocks increased in frequency from 1.2 per minute in the first half of the work period to 1.6 in the last; while in Condition II the increase was from 1.8 to 2.5. This is represented graphically in Fig. 2, where the whole work curves are plotted, on the basis of totals per half minute. There is an unmistakable tendency for blocks to increase in frequency whenever the task increases in difficulty.

The average length of the blocks is greater in the more homogeneous condition. In Condition I, the total average block length is 52.7, while in Condition II it is 74.4.

There is a greater increase in block length during the work period in the case of the more homogeneous condition. In Condition I, the total average length of the blocks is 48.1 in the first half, and 57.3 in the second half of the work period. In Condition II, it increases from 66.1 to 82.6. Figure 2 gives a comparison of the two conditions for each half minute of the work period, showing the steeper rise for Condition II.

When these results are taken in conjunction with those of Experiment I, with which they agree so strikingly, they give strong evidence for our initial assumption, that the blocks are rest periods which become increasingly more necessary as fatigue develops, but which compensate for it to such an extent that the decrement is in no case more than about ten percent in speed of reaction during a ten minute work period. In a predominantly muscular performance, like ergograph work, the decrement is often 100 percent in a like period, because no blocks occur to relieve the strain.

On the basis of these two experiments, two general principles of blocking can be formulated.

1. In the case of material containing conflicting elements, the frequency and length of the blocks are proportional to the number of such elements.

2. In the case of material in which the elements do not conflict, the frequency and length of the blocks are inversely proportional to the number of elements involved.

To these, a third tentative hypothesis might be added: If

it were not for the increased frequency and length of blocks, the decrement in competitive and highly homogeneous tasks would be much greater than it is.

No doubt there are other principles and conditions which affect the amount of blocking beside those already covered. It is still necessary to discover the cause for the tremendous increase which occurs in such abnormal conditions as stuttering, prolonged insomnia, extreme fatigue, under certain drugs, and in certain psychoses. It has been suggested, by Dunlap, that the reactions of subjects under low oxygen tension, when being examined for aviation service, show a tendency very similar to our blocking phenomenon. It may be possible eventually to tie all these symptoms together and relate them to some fundamental neuro-physiological principle. At present, however, the most valuable task seems to be to obtain more adequate and exact knowledge of what occurs on the behavior side.

What general conclusions can be drawn from the study regarding the causes of fatigue in mental work? The type of task involved in Experiment II, where such extreme homogeneity prevails, and where the subject knows exactly what is coming so that competition is largely removed, rarely if ever occurs in practical mental work. Yet it is only in such an artificial situation, where the sheer repetitive exercise of a single reaction system causes an actual temporary breakdown in that system, or cumulative refractory period, that the refractory phase analogy strictly holds. Therefore, Experiment I probably resembles more closely the conditions of actual mental work.

Here there is an increasing conflict among reaction tendencies, as the work progresses. There is not any complete breakdown in any one reaction system, but rather a slight lowering of responsiveness of certain systems relative to others, causing a shift in the readiness of different arcs to respond in their proper sequence in a coördinated series, and leading to conflict. Probably in practical work situations it is the relative refractoriness of certain arcs relative to others that causes the decrement in performance. But, in addition, there seems

to be a general coördinating or facilitating process, or function, corresponding to the traditional concept of 'attention,' which suffers a decrement. This coordinating process maintains the balance between the various reacting arcs; and when impaired, fails to overcome the incoordination resulting from the decrements in isolated systems. Let attention relax its vigilance ever so slightly, and a block occurs, or else an error or series of errors. A slight rest is given by the block, equilibrium is restored, and work proceeds smoothly again.

Summary.—Two experiments have been described, one of which demonstrates that the frequency and length of blocks in mental work are related to the amount of conflict between the elements in the task, the other shows that when such conflict is absent the frequency and length of the blocks are relative to the homogeneity of the task.

Blocking in Vocal and Manual Responses Compared

If, as has been suggested in previous articles, the tendency to block periodically in continuous mental tasks is a protective device which introduces frequent forced rests, thereby preventing excessive fatigue, it should appear in one response system as readily as in another. Manual responses should show blocks as abundantly as vocal responses. But there are two possible ways in which this might occur. On the one hand, if we assume that the periodic refractory condition, which causes the blocks, is entirely central and independent of the motor system involved, then the effect should be wholly transferable. That is, if, after developing a decrement in the vocal performance of a given task, one shifts to a manual performance of the same task, the decrement should carry over to the manual responses and the blocks should be correspondingly frequent. On the other hand, if the refractory condition is assumed to be partly central and partly specific to the motor system used, then the effect should be only partly carried over when one shifts from vocal to manual or from manual to vocal responses. Two questions are therefore raised. (1) How does the blocking in the manual performance of a task compare with the blocking in the vocal performance of that task?

(2) Do the decrement and increased blocking developed in one performance carry over completely or partially to the other performance?

To answer these questions, a study was made using a task which could be performed vocally or manually interchangeably. Thirty-two subjects, mostly university undergraduates, who had served in no previous experiment, were given the task of continuously reacting to colors arranged as in the Woodworth Wells color charts, *i.e.*, five colors arranged in rows in scrambled order. They reacted in one of two ways, vocally or manually. The former method consisted in naming the colors aloud, while the latter involved pressing the appropriate keys on a specially constructed keyboard. In both cases the reactions were recorded kymographically, and later analyzed for the presence of blocks and errors. Half of the subjects began with the vocal and half with the manual condition, to equalize practice. The following orders were therefore followed:

Order 1.	5 minutes manual	5 minutes vocal	5 minutes manual	5 minutes vocal
Order 2.	5 minutes vocal	5 minutes manual	5 minutes vocal	5 minutes manual

Each subject was given a preliminary short practice period to familiarize him with the test and with the two ways of reacting. He was then tested alternately for four periods of five minutes each as indicated above.

The results were analyzed to determine the following things: (1) the degree of correlation between the vocal records and the manual records in (*a*) reaction time, (*b*) frequency of blocks, (*c*) length of blocks; and (2) the degree of transfer of fatigue effects from one type of performance to the other, as shown by (*a*) a comparison of the first and third five minute periods as to initial and average level of performance, decrement in each, and increase or decrease of blocking. The second and fourth minutes were also compared. The point is that, for instance, by comparing the first five minutes vocal performance with the third five minutes vocal, one can determine whether the intervening manual task has affected the results and to what degree.

Results.—1. To compare directly the blocking tendency in the two response systems, rank order correlation coefficients were obtained between the manual and vocal records. A coefficient of 0.63 ± .07 found between the average number of responses per minute in the two performances indicates a good deal of similarity in the elements involved in the respective processes. But most investigators of reaction time have found fairly high correlations between different reaction times. More significant is a similarly high coefficient obtained, 0.62 ± .07, between average number of blocks per minute in the two reactions. And when manual and vocal records were compared for average length of blocks (computed as a function of the subject's average reaction time) a coefficient of 0.72±.06 was secured. In general, the subjects had considerably faster vocal reactions than manual, due in part to the strangeness of the manual key board. The former averaged 87 per minute, the latter 62. Blocking was slightly more frequent in the vocal performance, and the actual length of the blocks in seconds was less, but when block length was computed as a function of the subject's reaction time, the manual and vocal blocks were of identical length.

Thus far, the results seem to point to a close relation between the two types of performance, suggesting that the same process is involved in both—some central process which operates the same way regardless of the motor outlet. The blocking could then be assigned to the central process. But the further analysis disagrees with this conclusion.

2. To determine the transferability of decrements from one reaction system to the other, average work curves were obtained by averaging together the subjects' performance by minutes for each five minute period. This was done both for responses per minute and blocks per minute. The work curves of those who began on vocal were kept separate from those beginning on manual. They are shown below.

It will be noticed that considerable decrement developed during each five minutes work period. This is shown by a decrease in number of responses and an increase in number of blocks per minute. But the important thing is that the

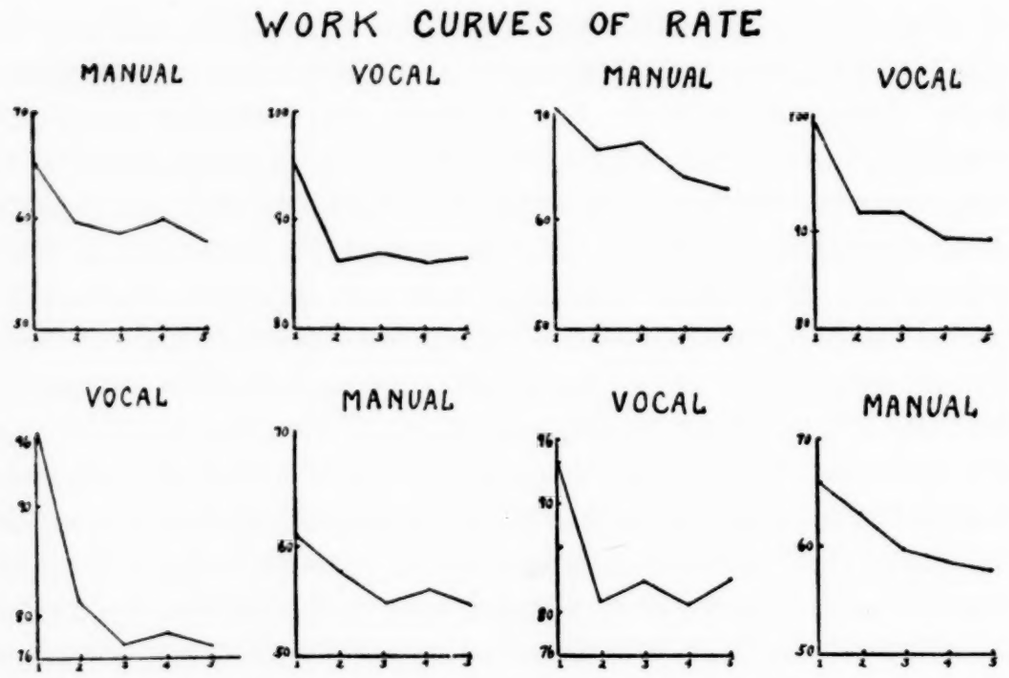

FIG. 3. Decrement transfer. Showing the degree to which decrement developed in one task transfers to the other. Abscissas represent minutes. Ordinates represent responses per minute.

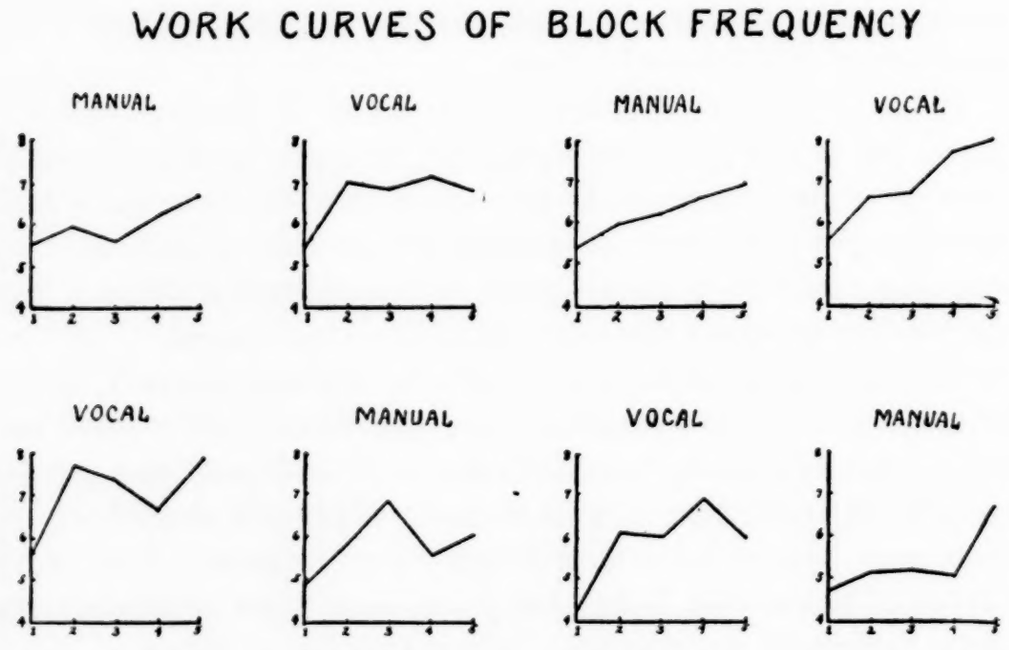

FIG. 4. Decrement transfer. Showing degree to which an increase in block frequency developed in one task transfers to the other task. Abscissa represents minutes; ordinate, responses.

decrement does not carry over to the opposite task. For example, the decrement developed in the manual task during the first five minutes is not augmented by the subsequent five minutes vocal work, but completely disappears so that the subject begins the manual performance in the third five minutes at a higher level than he began the first five minutes. The same is true of those who began on vocal. It is shown in the block frequency as well. The higher level is accounted for by practice, of course, but shows up only because the fatigue has disappeared during the performance of the intervening task. This can only be interpreted to mean that at least a large part of the decrement that develops is specific to the particular response system involved, either vocal or manual, and that the frequency of blocking is in part determined by this rather than by the periodic refractoriness of a common central process, such as perception or recognition, or an internal speech control assumed to operate in both cases. The blocks are in part motor blocks. The results are not conclusive until much longer work periods have been used and much greater decrements developed, so that rapid recuperation cannot hide the effect sought.

A study of the work curves for blocking compared with those for response rate shows much larger relative decrements in the former. Whereas the average loss in speed of responding is only 11.8 percent for 5 minutes, the average increase in block frequency is 32 percent, or nearly three times as much. This offers further confirmation of the theory that blocks, by increasing the amount of rest gained, enable the subject to maintain his speed and compensate for much of the fatigue.

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THE INFLUENCE OF DELAY IN THE AFTER-EFFECT OF A CONNECTION

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A satisfying after-effect occurring within a second or two after the connection of a response with a situation and belonging to it as its after-effect strengthens that connection by virtue of a 'confirming reaction', a physiological reinforcement which acts on connections as other forms of reinforcement do on sensations or movements [Thorndike et al., '32, '33, '33a]. A satisfying after-effect occurring after an interval of 5 sec. [Roberts, '30] or 15 sec. [Hamilton, '29, results by the obstruction method] was much less potent than one occurring immediately. A delay of 2, 4, or 6 seconds in an electric shock following a signal greatly retarded the transfer of an avoiding reaction from the shock to the signal, in Yarbrough's experiments ['21]. But in the first two cases the interval may not be empty. The animal may do something during it so that the after-effect does not belong to the connection between presence in the chamber and crossing the grid, or touching the rod. And in Yarbrough's experiments one cannot be sure just what the connections were, or just what the punishment of being shocked or the reward of not being shocked belonged to.

The intention of our experiments is to discover the influence of sheer empty delay, or of something closely approximating it which interferes as little as possible with the belonging of the after-effect to the connection.

Forty-eight educated adult subjects tossed balls back over their heads at an unseen target. Each subject was instructed as follows:

"Look at this target. You are going to throw balls like these at this target. The closer you get to the innermost or pink circle the higher your score. During this experiment you will earn money for good shots, and you will lose money for poor shots.

The money you earn will be paid you in cash at the end of the experiment. The unit of pay will be approximately equal to a French centime. If you do very well, it is possible to earn 30¢ or more over and above the pay for the hour. Sometimes when you throw a ball into the bull's-eye, I may tell you + 8, which means that you scored a bull's-eye hit and that, at the end of the experiment, I will give you in cash the equivalent of 8 centimes for each + 8 score I tell you. The blue circle next to it is the + 4 circle. Sometimes when you toss a ball into it, I may tell you + 4, which means that you tossed a ball into the second circle and that, at the end of the experiment, I will give you in cash the equivalent of 4 centimes for each + 4 I tell you. Sometimes when you throw a ball into the third circle, I may say zero, which means that the ball fell into the third circle and that, at the end of the experiment, I will neither give you, nor take away anything in money. Sometimes when you toss a ball into the fourth circle, I may tell you − 4, which means that your ball fell into the fourth circle and that, at the end of the experiment, I shall take away from your earnings the equivalent of 4 centimes for every − 4 I tell you. Sometimes when your ball falls outside any of these circles, I may tell you − 8, which means that you did not toss a ball into the bull's-eye or the second, or the third, or the fourth, and that, at the end of the experiment, I will take away the equivalent of 8 centimes for each − 8 that I tell you.

"Whenever you toss a ball you will sit on this stool, with your back to the apparatus. You will not see the target. You will toss the ball when you hear a click like this. Every time you hear a click, you will toss the ball over your shoulder trying to hit the bull's-eye.

"Remember that + 8 means bull's-eye, + 4 means second circle, 0 means third circle, − 4 means fourth circle, and − 8 means outside the four rings.

"It is your job to throw the ball as close to the bull's-eye as you can so as to get a higher score and get more money. Is that clear?

"You will now throw some practice shots which will not count. These shots are to make sure that you understand what to do. Remember, throw a ball whenever you hear a click. (Here followed a few practice shots to check the understanding of the directions.)

"You will now throw forty balls, one each time you hear a click. I will not tell you your score. (Here followed a series with no announcements of 'Right' or 'Wrong.')

"You will now throw a ball each time you hear a click. I will tell you as soon as the ball strikes where your ball landed. Remember, the order is + 8, + 4, 0, − 4, − 8, from bull's-eye to outside of the target. (Here followed the 'Immediate' Series.)

"You will now throw a ball each time you hear a click. I will tell you your score before I give you the click for the next throw. When you hear the score, get ready for the next shot, but only throw the ball when you hear the click. (Here followed one or more series with delay. The score was announced 1 second after the ball struck the target, 2 seconds after, or 4 seconds after, or 6 seconds after, according to the series that was being operated.)

"You will now throw a ball each time you hear a click. I will only tell you your score for a toss after you've thrown another ball. In other words, after you throw your first ball, I will give you a click to throw a second ball. After you have thrown this second ball, I will tell you what you scored on your first throw. When you have thrown the third ball, I will tell you what you scored on your second shot, and so on until you've tossed forty and one balls. Is that clear? (Here followed a series with the reward referring to the shot one step back in the series.)"

The target consisted of a wire frame of concentric circles. The inner circle or +8, or bull's-eye, had a diameter of 8″. The +4 circle had a diameter of 16″; the 0 circle had a diameter of 24″, and the −4 circle, 32″. The outer frame was 80″ square, from which extended 24″ cambric all around so adjusted to catch all −8 balls. Pockets were formed by sewing colored cambric tightly over the frame. Depth of the pockets so-formed was 12″. The entire target was raised 18″

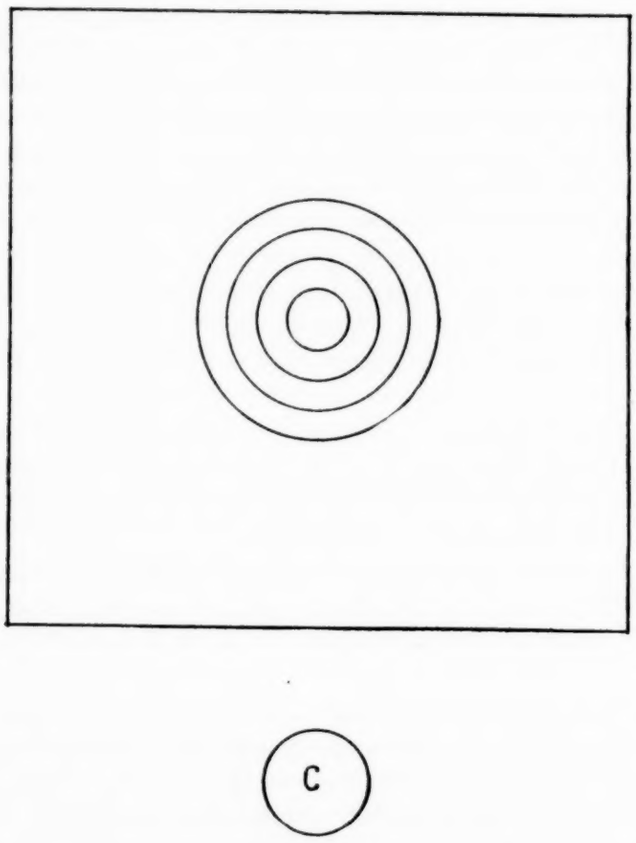

FIG. 1. The target and the stool (*C*) on which the subject sat.

from the floor so that the horizontal plane of the top of the target was 30″ from the floor. The subject sat facing away from the target on a stool 18″ high fixed at 60″ from the center of the target and tossed the balls back over his head or shoulder.

The balls that were tossed at the target were wooden beads, perforated oblate spheroids with a long diameter of 13 mm and a short diameter of 12 mm. The 3 mm perforation was through the long diameter. The average weight of each bead was .61 gram.

There was no auditory cue possible of correct interpretation as to location. When the ball fell on the cambric, the light sound was equally uninformative for all scores.

Groups *A* and *B* each consisted of twelve subjects, candidates for degrees at Teachers College who participated in the experiment during Christmas week, 1931. Groups *C* and *D* each consisted of twelve subjects, sampled in the same way as Groups *A* and *B* who participated in the experiment during January, 1933.

The program for Groups *A* and *C* was as follows:

Day 1.	40	throws, no announcement of results
	40	" announcement immediate
	40	" " after 1 second
	40	" " " 2 seconds
	40	" " " 4 "
	40	" " " 6 "
	40	" " " next throw
Day 2.	40	" " " " "
	40	" " " 6 seconds
	40	" " " 4 "
	40	" " " 2 "
	40	" " " 1 second
	40	" " immediate
	40	" no announcement of results
Day 3.	120	" announcement immediate
	120	" " after 6 seconds
	40	" no announcement

Since the time from the throw itself until the ball hit the target averaged about 1¼ seconds, the effective delays ranged from about 1¼ seconds to about 7¼ seconds.

For Groups *B* and *D* the program was changed to the following:

Day 1.	40	throws, no announcement of results
	40	" announcement after 6 seconds
	40	" " " 4 "
	40	" " " 2 "
	40	" " " 1 second
	40	" " immediate
	40	" " after next throw

Day 2.	40	throws,	announcement after next throw
	40	"	" immediate
	40	"	" 1 second
	40	"	" 2 seconds
	40	"	" 4 "
	40	"	" 6 "
	40	"	no announcement of results
Day 3.	120	"	announcement after 6 seconds
	120	"	announcement immediate
	40	"	no announcement

We are thus in possession of facts by which any large influence of delay in the reward or punishment may be observed and roughly measured.

The observations and measurements may be of various forms. Perhaps the most informing is to measure the difference between the score in the last 4 trials of a 40 with a certain delay and the last 4 trials of the preceding 40 (of the same day) of whatever sort that was. Doing this for the records of Day 1 and Day 2 we have the results shown in Table 1. The

TABLE 1

GAINS MEASURED BY COMPARING THROWS 37 TO 40 WITH THROWS 37 TO 40 OF THE PRECEDING 40 OF THE SAME DAY: USING DAYS 1 AND 2

Each entry is the gain in average score for one throw due to the practice from 40 throws, 2 of the preceding type and 38 of the specified type.

	Immediate	1 sec	2 sec	4 sec	6 sec
Groups *A* and *B*.....	.15	.83	.25	.06	.88
Groups *C* and *D*.....	1.02	.49	− .06	− .42	1.31
All.....	.58	.66	.09	− .18	1.09

maximum score possible is 8.0; the minimum − 8.0.

Measures of the learning during any 40 that are freer from accidental variations may be had by using as the end-point the average of the score in the last four of that forty and the score in the first four of the following forty (if it comes on the same day) and as the beginning point the average of the score in the first four of the forty the influence of which is in question and the last four of the preceding forty (if it came on the same day). Doing this for the records of Day 1 and Day 2 we have the results shown in Table 2. The average gains for 0, 1, 2, 4 and

TABLE 2

GAINS MEASURED BY COMPARING THE AVERAGE OF EIGHT THROWS (37 TO 40 WITH THE SPECIFIED DELAY AND 1 TO 4 OF THE SUCCEEDING 40) WITH THE AVERAGE OF EIGHT EARLIER THROWS (37 TO 40 OF THE PRECEDING 40 AND 1 TO 4 OF THE 40 WITH THE SPECIFIED DELAY): USING DAYS 1 AND 2

Each entry is the gain in average score per throw due to the practice from 40 throws.

	Immediate	1 sec	2 sec	4 sec	6 sec
Groups *A* and *B*.....	.38	.63	.57	− .31	.94
Groups *C* and *D*.....	.65	.17	− .21	.18	.91
All.....	.52	.40	.18	− .07	.92

6 sec delay by the two sorts of measure are, respectively, .55, .53, .14, −.13, and 1.01.

As a special case of short *versus* long delay we may use the Day 3 records in which 120 consecutive throws were with the immediate and 120 with the 6 seconds announcement. In this case we may keep free from any effect of the 24 hour intervals by using the first 4 or the first 8 throws of the sort in question as the beginning score. The end score may be (1) the last eight throws of that same sort or (2) the first eight throws following them or (3) the average of the last 4 of the same sort and the first 4 of the next sort. Each of these six measures is impartial as regards the 0 delay and 6 sec delay. The results by all six methods appear in Table 3. The average gain for 0 delay is .31; that for 6 sec delay is .24.

On the whole, there is no proof that delays up to 6 sec weaken the influence of after-effects. By any reasonable weighting of the results from days 1, 2 and 3, the 6 sec delay appears as beneficial to learning as 0 delay. If we weight the results from the 120 throws on day 3 as 1½ times as important as the results from the 80 throws of days 1 and 2 combined, we have for 0, 1, 2, 4, and 6 sec delay, gains of .40, .53, .14, −.13, and .55. The gains for 0, 1 and 2 sec are greater than for 4 and 6 sec, but the progression is not at all regular and the difference is not reliable, in spite of the large number of subjects.

It should be noted that improvement in this experiment

TABLE 3

GAINS MADE IN 120 THROWS ON DAY 3 ACCORDING TO SIX METHODS OF MEASURING THE GAIN

	0 delay						6 seconds delay					
	1–4 as beginning			1–8 as beginning			1–4 as beginning			1–8 as beginning		
	113–120 as end	1–8 of the next as end	Average of 117–120 and 1–4 of next as end	113–120 as end	1–8 of the next as end	Average of 117–120 and 1–4 of next as end	113–120 as end	1–8 of the next as end	Average of 117–120 and 1–4 of next as end	113–120 as end	1–8 of the next as end	Average of 117–120 and 1–4 of next as end
Groups *A* and *B*	.42	.24	.39	.34	.16	.31	.10	−.13	−.03	.29	.04	.12
Groups *C* and *D*	.50	−.02	.41	.51	−.01	.42	.66	.20	.40	.65	.19	.39
All	.46	.11	.40	.43	.08	.37	.38	.03	.18	.47	.11	.27

may be due to bodily posture as well as to the actual movements of arm and trunk and that posture may shift slowly so that the 'confirming reactions' due to the satisfying announcements may find the more desirable postures nearly as often after 6 seconds as after 0 or 1.

When the announcement is made after an interval filled by another throw and refers back to the previous throw there is no gain at all. Using measures comparable to those of Tables 1 and 2 (except that we have to use 4 trials from a previous day or succeeding day in the computations), we find an average change of −.16 in 40 throws (−.24 by the method used in Table I and −.08 by the method of Table II). Combining methods but separating groups, we find average changes of −.01, −.33, −.19, and −.11 for groups *A*, *B*, *C*, and *D*, respectively. The probable error of the −.16 is .04.

When no announcement at all is made, there is an average change of −.71 in 40 throws. The probable error of the −.71 is .42.

In spite of records from forty thousand throws, all that can safely be said is that there is improvement when announcements follow the connections with no intervening throw, even with delay up to 6 seconds, that delays of 4 and 6 sec seem to lessen this improvement, that some of this improvement is lost during periods of no announcement, and that announcements referring to the second throw previous do no good. The experiments support the view that there is no learning without a confirming reaction, that this has no power to seek out and strengthen the 'right' connection, but strengthens primarily what connection has just been operating, and that the mere lapse of time up to 6 sec leaves a connection still fairly sensitive to influence from the confirming reaction.

Very much more decisive results statistically could be obtained by experiments using more available and definable responses, such as tapping keys, saying words or numbers, etc., but it is almost impossible to prevent human subjects from keeping a connection ending in such a response in mind until the after-effect occurs or from recalling it to mind when the after-effect occurs and then repeating some equivalent of the

after-effect. The real relation would then be immediate, regardless of how long an ostensible delay we used.

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A GRADIENT IN INCIDENTAL LEARNING

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Introduction.—The expression 'incidental learning' as used in this writing should be interpreted thus: 'learning without obviously specific motivation.' Here it is implied that all learning depends upon motivation. To be sure one is often embarrassed to discover and define motives. The present study provides a case in point.

Various factors have been demonstrated to be effective in the phenomena of serial learning order. As will appear, the conditions of the present study probably rule out most of these well known factors. The simple experiment to be reported consists of testing the members of two large classes in elementary psychology for their retention of their respective rolls. The specific objective was to investigate retention for the order of the roll call as a function of serial position relative to the individual's own name. Insofar as the author is aware there exists no report of a similar study.

Subjects.—The experimental groups *A* and *B* represented the populations of two large lecture sections in elementary psychology. Group *A* contained one hundred and six students. Group *B* contained sixty students. These classes met three times a week, upon Tuesday, Thursday and Friday.

Procedure.—1. The alphabetized roll was called at the beginning of each meeting of the class for fifteen weeks; thus forty-five repetitions were accomplished.

2. Care was taken to keep the roll call procedure as constant as possible.

3. At no time preceding the test period were the groups aware of taking part in an experimental procedure.

4. At the beginning of the forty-sixth meeting of each section the following instructions were read:

"Prepare a large sheet of note paper with consecutive numbers from one to one hundred and six (or one to sixty). Make sure that there is space to write a single word opposite each number. Write your own name on the back of the sheet." As soon as all were so prepared the instructions were continued as follows. "This is an experiment and not an examination so please refrain from coöperative organization and effort. I wish to find out how well you remember the order of the roll-call as it has been repeated daily. I shall read the first name. Then I shall give you five seconds to write the name which follows the first name in the roll. Write it after number two on the sheets you

have prepared. After five seconds have elapsed I shall read the second name and give you five seconds to write the third name. In this manner we shall go all the way through the roll. I shall read the names at five-second intervals and you, at each reading, will try to write the immediately following name. If you do not know a name write what you think it is. If you have no notion at all just leave the space blank. Do not waste time upon trying to spell names correctly and do not correct your papers in any way." The roll was then called according to this procedure and the papers were collected.

Treatment of the Data.—In the treatment of the data absolute serial position was disregarded. Each individual's paper was scored for ten serial positions relative to the individual's own name. These relative positions were the first, second, third, fourth and fifth preceding and the first, second, third, fourth and fifth following. This treatment obviously excluded five individuals at each of the two extremities of the roll. Thus the populations were reduced to ninety-six and fifty. The practice of reading the first name in each case further reduced the populations to ninety-five and forty-nine. The scoring was accomplished in the simple terms of right or wrong. This procedure obviously ruled out such possibly influential factors as primacy, finality, unusual names, well known names, etc. Also, it is doubtful if students use such mnemonic devices as arbitrary and logical associations, rehearsal, counting, etc., in the accomplishment of such learning as is reported in this study. The data were then massed in tabular form (see Table I) and the graphs presented

TABLE I

RETENTION OF THE ORDER OF THE ROLL CALL AS A FUNCTION OF SERIAL POSITION RELATIVE TO THE INDIVIDUAL'S OWN NAME

		Relative Serial Positions										
		Preceding					Own Name	Following				
		5	4	3	2	1	0	1	2	3	4	5
Group *A*, *N* = 95	No. correct anticipations...	22	37	38	44	64	85	41	25	20	20	14
	Percent.....	23.2	38.9	40.0	46.3	67.4	89.5	43.2	26.3	21.1	21.1	14.7
Group *B*, *N* = 49	No. correct anticipations...	12	9	13	19	27	45	14	14	10	8	10
	Percent.....	24.5	18.4	26.5	38.8	55.1	91.8	28.6	28.6	20.4	16.3	20.4
A and *B* combined	Percent $\frac{A(\%)+B(\%)}{2}$	23.9	28.7	33.3	42.6	61.3	90.7	35.9	27.5	20.8	18.7	17.6

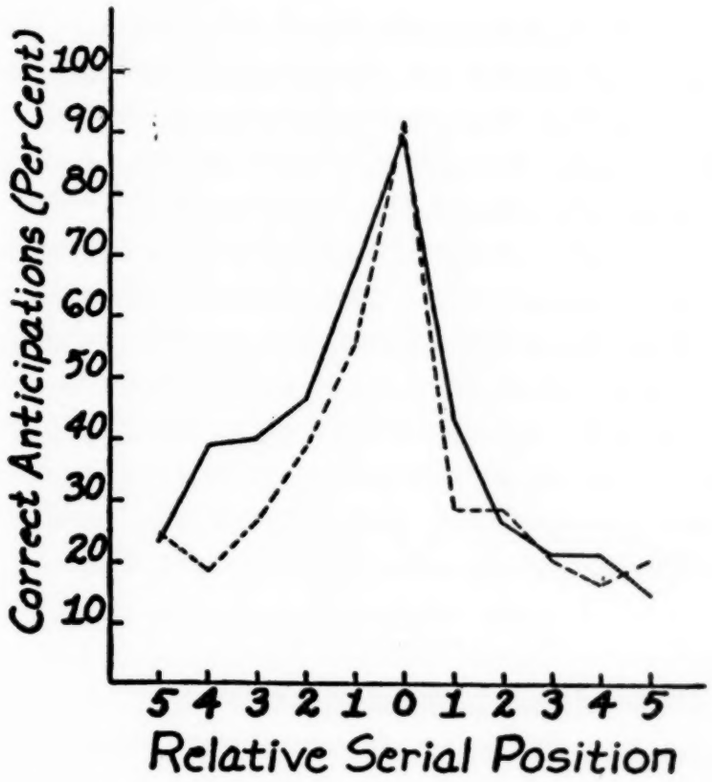

FIG. 1. Retention of the order of the roll call as a function of serial position relative to the individual's own name.

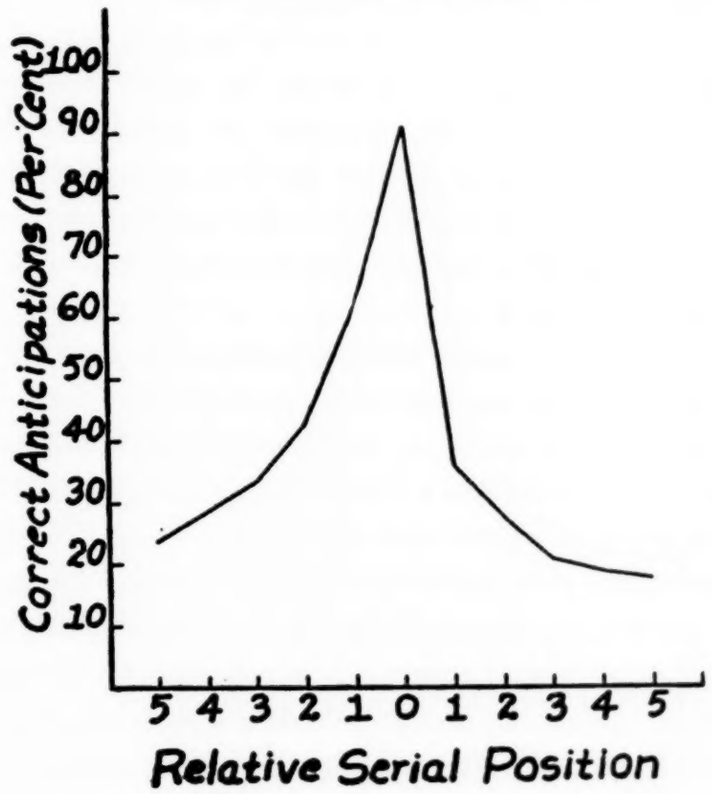

FIG. 2. A combination of the two graphs presented in Fig. 1.

in Fig. 1 were constructed from this table. Table I also contains a combination of the results from the two groups. Figure 2 represents this combination graphically.

Results.—Refer to the solid line graph in Fig. 1. The height of the ordinate at the abscissa scale point marked 0 represents the percentage of the Group *A* population who correctly anticipated their own names. The value is 89.5 percent. The immediately preceding ordinate designated by 1 and having a value of 67.4 percent represents retention for the first *preceding* serial position relative to the individual's own name. The other *preceding* values read regressively are: 46.3 percent, 40.0 percent, 38.9 percent, 38.9 percent and 23.2 percent. These values are to be interpreted in a similar manner.

The ordinate immediately *following* the zero point and having a value of 43.2 percent means simply that 43.2 percent of the population of Group *A* correctly anticipated the names which immediately followed their own names in the roll. The other *following* values read progressively are: 26.3 percent, 21.1 percent, 21.1 percent and 14.7 percent. These other points are to be interpreted in a similar manner. The graph as a whole represents retention of the order of the roll as a function of serial position relative to the individual's own name. The broken line graph in Fig. 1 is a similar graph derived from the data gathered in the experiment concerning Group *B*. The graph in Fig. 2 represents a combination of the results from the two groups.

Discussion and Interpretation.—Notable in Fig. 1 are the two reasonably regular gradients appearing in each of the line graphs. There appears a positively accelerated gradient preceding the individual's own name and a negatively accelerated gradient following. Further, the retention at each point preceding the individual's own name is superior to retention at the corresponding relative serial position following. The positively accelerated gradient preceding the individual's own name conforms in both direction and shape to the demands of Hull's *goal gradient hypothesis* (**2**). To interpret this gradient according to the goal gradient hy-

pothesis one must assume that the individual's anticipation of his own name in the experiment and his reaction to his own name in the daily calling of the roll constitute goal reactions. The preceding segments then become increasingly significant behavior units preparing the organism for the goal reaction. The author is not prepared to discuss the probable physiological nature of this preparation. Hull speaks of an 'excitatory goal gradient' (**2**). In a recent study he describes a 'speed-of-locomotion gradient' (**3**, 417) and suggests that it is a function of an 'excitatory goal gradient.' Referring to the present data one might speak of a modifiability gradient, or a docility gradient, or an excitatory gradient or possibly, but not very meaningfully, an attentivity gradient. Whatever it is named, the author wishes to suggest that the positively accelerated gradient preceding the individual's own name may represent, in relatively pure form, a goal gradient as conceived by Hull (**2**). The author suggests 'relatively pure form' because the nature of the present experiment probably excludes factors which have been judged to distort the goal gradient in previous experiments, for example the recent experiment by Hull (**3**).

The author chooses to interpret the negatively accelerated gradient following the individual's own name as representing the recession of the variables which produce the preceding, positively accelerated gradient. This might also be called a goal gradient. Rat maze experiments have no way of revealing such a gradient. Nevertheless, it may represent something of what happens upon the attainment of any goal by an organism. In Hull's technical language, this negatively accelerated *post-goal gradient* may represent the recession of the potency of the 'drive stimulus' or of the 'goal stimulus' (**1**) or of both. In other language, it may represent what happens within the organism when it 'loses interest' in, or 'set' for, a problem solved, a goal attained. Much has been written about *change of set* and *loss of set* but insofar as the author is aware no one has suggested the shape of the curves of these functions. It is interesting to note that the curve discussed here resembles the conventional curve of forgetting.

The third aspect of the data noted above, that is, the generally superior retention for the relative serial positions preceding the individual's own name, appears to demand no interpretation beyond that implied by the above discussion of the post-goal gradient. A recent study by Thorndike (4) reveals a phenomenon which may be related to the phenomena reported here. On page 55 of this study there appear two graphs which resemble the present ones. Thorndike's graphs represent the varying effect of a rewarded act in a series of acts upon punished acts in this series as a function of serial position relative to the rewarded act. The study shows that the effect is greatest for the serial positions *following* the rewarded act. Referring to the present study we might consider the individual's anticipation of his own name as a uniquely rewarded act. If this be legitimate then the factor revealed by Thorndike's study may be operating. However, the data from the present study show that the resultant facilitative effects are relatively greater for the serial positions *preceding* the unique act. Therefore, if the irradiated 'satisfying after-effect,' as demonstrated by Thorndike, is operative in the present study, it is obscured by other and more potent factors.

The interpretations suggested in this section give rise to interesting implications, especially with regard to the nature and phenomena of 'set.' It is not within the scope of this paper to discuss these implications.

Summary and Conclusion.—A simple experiment, wherein the members of a class of one hundred and six undergraduates are tested for their retention of the order of the roll call, is reported. When the data have been corrected for such factors as primacy, finality, unusual names, well known names, et cetera, there appears a significant distribution of correct reactions. When the individual's own name is used as a point of reference, there occurs a positively accelerated, rising gradient of correct reactions for the names immediately preceding the individual's own name in the order of the roll call. Further, there appears a negatively accelerated falling gradient for the names immediately following the individual's own name. This result has been confirmed by using the same procedure

with another large class. By way of interpretation it is suggested that the gradient preceding the individual's own name may represent a relatively pure form of the goal gradient as demanded by the *goal gradient hypothesis* of Hull (**2**). The gradient following the individual's own name is interpreted as a *post-goal gradient* representing the recession of the variables which produce the *pre-goal gradient.* These interpretations are offered only as being suggestive. Considered as hypotheses they may, and do, generate certain inferences which are accessible to experimental investigation.

(Manuscript received December 14, 1934)

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SOME OBSERVATIONS ON THE RELATION OF THE PRINCIPLE OF PHYSIOLOGICAL POLARITY AND SYMMETRY AND THE DOCTRINE OF CEREBRAL DOMINANCE TO THE PERCEPTION OF SYMBOLS

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Orton (**5**), a pioneer worker in the field of word-blindness (*strephosymbolia*), has maintained that the tendency of many children to reverse and invert written symbols is related to a lack of a sufficient margin of dominance of one of the cerebral hemispheres over the other. According to his view the mnemonic images governed by the two hemispheres are not in the same orientation but on the 'third functional level' are mirrored counterparts of each other. The human organism, being symmetrical about the median plane but polaric about the transverse and horizontal planes, is predisposed to make similar responses to reverse patterns of stimulation if they are presented on opposite but counterpart sides of the median plane. Lack of dominance of one hemisphere over the other makes difficult the differentiation of one pattern from the other.

Any reference to the spatial orientation of mnemonic images is likely to provoke the criticism that images are not spatial phenomena but are physiological processes. The answer is that the physiological processes are really serial processes in spatially distributed organs and hence have the property of spatial direction.

In Fig. 1 let us first suppose that the left hemisphere is dominant when a stimulus '*d*' is presented. Let us suppose that the resulting percept is indicated by the arrow marked, '1.' Of course it would be improper to assume either that the contour or that the spatial orientation of the percept is like

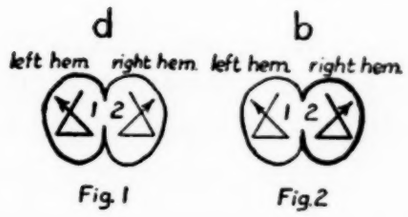

Fig. 1

Fig. 2

Natural

Fig. 3 Reverse and Inverse

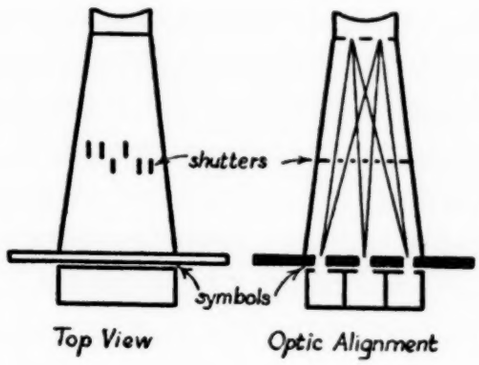

Fig. 4

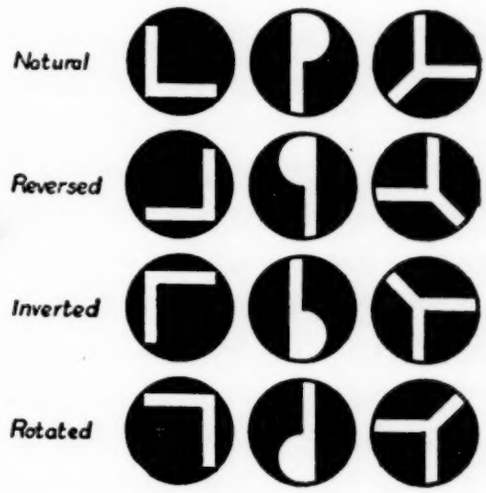

Fig. 5

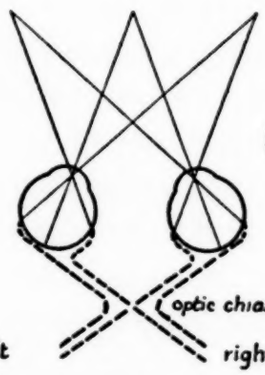

Fig. 6

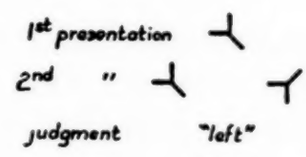

Fig. 7

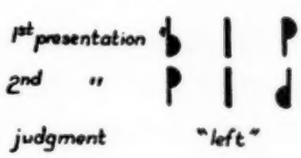

Fig. 8

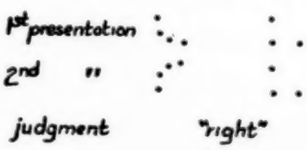

Fig. 9

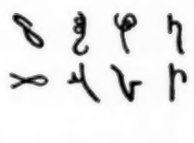

Fig. 10

that of the stimulus; one can only say that whatever contour or spatial orientation the percept possesses *stands for* or *symbolizes* the *stimulus in its relation to the organism.* In recognition of this fact the percept has been represented in the diagram by an irregular arrow. Although the left cerebral hemisphere is the seat of the highest authority in this gradient the dynamic unity of the entire nervous system must not be overlooked. Obviously there must be some related process in the right cerebral hemisphere. Travis (**6**) has been successful in showing this experimentally. If the process in the left

hemisphere is principally in a peripheral or abductive direction as indicated by the arrow '1' the physiologically symmetrical process in the right hemisphere is also in a peripheral or abductive direction as indicated by the faint arrow marked, '2.'

Now let us suppose that because of fatigue or lack either of inherent or cultivated bias the left hemisphere becomes recessive and the right hemisphere becomes dominant. A new gradient is set up in which the highest authority is located in the right hemisphere. Let us suppose that the stimulus '*b*,' a spatially reversed stimulus, is presented. *In relation to the new gradient* this stimulus calls for another peripheral or abductive process and the resulting percept may be indicated by the heavy arrow '2' in Fig. 2. Physiologically the arrows in both figures correspond to each other, all representing abduction, hence the stimuli '*d*' and '*b*' are judged congruous.

We have attempted briefly to explain the relation of the doctrine of cerebral dominance to the perception of the spatial orientation of symbols on the basis of similar processes in the two hemispheres. We have argued that since a percept consists of serial processes in spatially distributed organs it may have a spatial trend or direction. Our explanation would be even less awkward if we turned behaviorist and assumed that percepts were to be identified by the corresponding movements of peripheral structures, gestures of the limbs, articulatory movements of the vocal mechanisms, etc.

Before proceeding further with a discussion of theory it would be well to summarize the facts to be explained. There is an infinite number of positions or orientations in which a symbol may be presented. The orientations may be divided into two groups, a natural series and an inverse series. In each series the symbol may be moved from one position to another by rotation. Fig. 3 illustrates the difference between these two series. On the basis of symmetry about the median plane 1A would be confused with 1B, 2A with 2B, 3A with 3B, etc. On the basis of symmetry about the horizontal plane 1A would be confused with 5B, 2A with 6B, 3A with 7B, etc. On the basis of symmetry about both median and horizontal planes 1A would be confused with 5A, 2A with 6A, 3A with 7A,

etc. Since the doctrine of cerebral dominance accounts only for symmetry about the median plane it implies that only symbols in corresponding positions in different series should be confused. Is this the case? According to some reports it is not. Cases of word-blindness have been observed in which both reversed and inverted handwriting have appeared. One observer, Bluemel (**1**), discards the doctrine of cerebral dominance principally upon this evidence. We have found in our own clinical experience that some children with reading disabilities possess an unusual facility in reading inverted and rotated symbols as well as reversed symbols. Obviously this cannot be explained upon the basis of symmetrical processes or gestures about the median plane alone. Confusion of inverted symbols would indicate symmetrical processes about some horizontal plane. Confusion of rotated symbols would indicate symmetry about both median and horizontal planes at the same time. One would then suppose that confusion of rotated symbols would be the least frequent; and indeed that seems to be the clinical experience. One thing now appears to be clear; if we are to explain confused perception of the spatial orientation of symbols on the basis of symmetrical physiological processes or gestures we must consider both median and horizontal planes as axes of symmetry.

There are further facts to be explained. Whereas the preceding discussion has been concerned with spatial orientations there are clinical observations of temporal confusions to be considered. Temporal confusion, if explained upon the basis of symmetrical processes, implies symmetry about a point in time. That is to say, if I am to smooth my hair, pull my ear, and open my mouth the symmetrical series of gestures is to close my mouth, push my ear, and ruffle my hair. Travis (**6**) has shown temporal confusion in the breathing movements and neural action currents of stutterers. He has shown that the stutterer frequently tries to speak during inspiration and that some muscles are acting as if the individual were inspiring at the same time that other muscles are acting as if the individual were expiring. A clinical case has come to our attention which illustrates the point. A fourteen-year-old girl

frequently writes the last letter of a word first, then the next to the last letter, and so on. Many words are pronounced as if they were spelled backwards. This is symmetry about a point in time, not about a plane in space, and yet in this same case we have symptoms which indicate a spatial symmetry of response about the median plane. Frequently she will change her chalk or pencil from one hand to the other after starting to write the letters of a word in reverse order and rewrite the word correctly. For instance, in writing the word 'little' she wrote with the left hand 'e l t,' then hesitated, changed the chalk to her right hand, and wrote 'l i t t l e.' Here it appears as if temporal confusion is related to spatial confusion about the median plane; but what theory, mechanistic or otherwise, accounts for such a fact?

We have been able to show elsewhere (4) that reading disability is related to disorders in the rhythm of speech and have suggested that on the functional side the common factor between the two types of disorders is a transient aphasia. This transient aphasia also is related to the lack of cerebral dominance. Any transient disorder immediately suggests problems of refractory phase lengths and mental fatigue. If symmetry about both median and horizontal planes, symmetry about a point in time, and refractory phase are all related to stuttering, word-blindness, and transient aphasia about all that can be advanced by way of explanation is some doctrine of general neural disintegration—which may mean almost anything.

Before attempting further explanation of the confusion of differently oriented symbols it seems advisable to consider three questions. (1) Is cerebral dominance related at all to the perception of differently oriented symbols? (2) Are gestural responses of the adductor-abductor type always used in the perception of symbols? (3) What kind of change in the orientation of symbols is most likely to cause confusion?

We have undertaken a series of experiments in order to throw light upon these questions. For two of these experiments a special piece of apparatus has been constructed (Fig. 4). This apparatus is a tachistoscope by means of which

short time exposures can be made in various parts of the field of peripheral vision separately for each eye. It consists of a light-proof box into which the observer looks. Symbols are presented by lighting lamps placed behind appropriately formed translucent screens. A system of shutters enables the experimenter to control the exposures to either eye independently and without the knowledge of the observer. A timing device consisting of a pendulum with movable counterbalances and sliding mercury cup drop switches enables the experimenter to make presentations in any order and for any period of time.

In the first experiment there were three symbols used (Fig. 5). Each was presented in one of the four possible orientations, natural, reversed, inverted, and rotated 180°. In each observation the observer was allowed to fixate with both eyes a symbol in the median plane for a period of 1700 σ. At the end of this period the light behind the symbol was turned off and the lights behind two side symbols situated so as to stimulate non-foveal retinal areas were turned on for a period of 100 σ. This brief presentation of the side symbols should have made it difficult for the observer to move his eyes into a position of fixation on either side symbol since the reaction time for light is probably considerably greater than 100 σ. In each observation the shutters were so arranged that both eyes could be fixated on the median symbol but only one eye could be stimulated by each of the side symbols. Figure 6 illustrates the retinal areas, principal neural paths, and hemispheres involved.

In each observation the side symbols were similar in form to the median symbol. The observer was asked to report whether the left, the right, both, or neither of the side symbols was in the same orientation as the median symbol (Fig. 7). The presentation series was so constructed that each symbol in all of the possible combinations of orientations was presented through each of the four neural tracts. Records were kept of the conditions under which errors were made. 18 observers, men and women, graduate and undergraduate students in psychology made 72 observations each.

An attempt was made to control the factor of manual preference since this is supposed to be a clue to cerebral dominance. 13 observers reported that they were right handed and 5 reported that they were left handed. When the phi-phenomenon technique (Jasper (**3**)) was used as an objective check 7 observers indicated right central dominance, 7 indicated left central dominance, 3 indicated right peripheral dominance, and 1 indicated left peripheral dominance. There was no correlation between the reports of handedness and the phi-phenomenon criterion; furthermore both left and right handed groups showed similar reports in the experiment. No further attempt was made to control the factor of cerebral dominance among the observers. The factor of cerebral symmetry, however, continued to be an object of investigation.

There were 314 errors of judgment distributed as follows: 70 when the principal neural path lay from the right eye to the left hemisphere, 66 when it lay from the left eye to the left hemisphere, 73 when it lay from the right eye to the right hemisphere, and 105 when it lay from the left eye to the right hemisphere. The greatest number of errors was made when the principal neural path lay through those organs which are considered as usually non-dominant. Statistically, however, the mean number of errors made under this condition differed from the mean number made under the first condition only by about 2½ times the probable error of the difference between the means. This difference may be interpreted to be somewhat statistically significant but by no means conclusive.

About all that can be concluded from this experiment is that among observers in whom we have reason to believe there is a normally well established neural integration there is a tendency to make errors in detecting differences in the orientation of symbols when the non-dominant eye and hemisphere are involved in the principal neural tract. Our first question cannot be answered satisfactorily but there remains some evidence for believing that there is a relation between cerebral dominance and the perception of symbol orientation.

The second question, are gestural responses of the adductor-abductor type always used in the perception of symbols, is

difficult to answer because gestural responses are not easily controlled. Our experiments yielded four kinds of evidence on this point. In the first place the observers reported that they 'kept the symbols straight' by moving their hands or pointing their fingers to the right or left while fixating the median symbol. By maintaining this posture they were able to make a direct comparison with the side symbols in the second phase of the observation. Some observers verbalized, using such expressions as '*q*,' '*p*,' '*y* leaning to the left,' 'down right,' etc. We suppose that this is another case where the conscientious behaviorist is tempted to accept introspective evidence.

In the second place we may consider the answer to our third question together with the answer to the second and call attention to the conditions of orientation under which errors were made. There were 51 errors in which symbols in reverse orientation were judged as in the same, 37 in which symbols in inverted orientation were judged as in the same, and 27 in which symbols in rotated orientation were judged as in the same. The number of presentations was the same in each instance. From this it appears that reversals are most confusing and rotations least confusing. Support for this same conclusion is obtained from the following: When symbols in the same orientation were presented with symbols in reverse orientation there were 84 errors, when presented with symbols in inverted orientation there were 62 errors, and when presented with symbols in rotated orientation there were 53 errors.

If we consider rotated symbols as symbols which have been both reversed and inverted we may say that while gestures of the adductor-abductor type might have been used to identify reversals and gestures of the elevation-depression type might have been used to identify inversions either kind of gesture could have been used to identify the rotations. The infrequent misjudgment of rotations might be due to the double check which the observer might apply to them. The frequent misjudgment of reversals fits very neatly into the argument in favor of the doctrine of cerebral dominance. The misjudg-

ments of inversions might be explained upon two grounds; first, the observer would have to depend only upon one kind of gesture, the elevation-depression type, or second, the tendency to make the initial gesture with one hand or on one side might confuse the observer when a symbol in the second phase of the presentation appeared on the opposite side of the body. In the latter case there would have to be a transfer of gesture across the median plane. This in itself would be confusing. All of this, then, is consistent with the view that gestures are perceptual concomitants.

A third kind of evidence, however, casts doubt upon the gestural hypothesis. This evidence was gotten by varying the experimental series so as to discourage the use of gestures. This was done by requiring the observer to keep in mind two symbols at a time instead of one. The same apparatus was used. In the first part of each presentation a fixation light was presented in the median plane. After 1700 σ the light was removed and two side symbols presented for 100 σ. After an interval varying from 5 to 8 seconds the fixation light was presented again and followed by two side symbols. (Fig. 8). If both of the second pair of symbols were the same as the first pair the observer reported, 'both'; if neither, 'neither'; etc. Series were arranged so as to balance the different combinations of eyes, orientations, symbols, and sides of presentation. 18 observers, undergraduate students in psychology, were used as observers. All reported dextrality, or right handedness. Records of the conditions under which errors were made may be summarized as follows: There was an average of 4.28 errors in judging symbols in the same orientation, an average of 4.34 errors in judging symbols in reversed orientation, an average of 5.66 errors in judging symbols in inverted orientation, and an average of 4.21 errors in judging symbols in rotated orientation. (The first and last figures were corrected for an attenuation logically arising out of the nature of the presentation material.) The results of this experiment may be summarized by saying that when the use of gestural cues was discouraged by complicating the presentations only the inversion remained particularly difficult to

identify. The hypothesis of symmetrical gestures or processes about a median plane will not explain this fact.

A variation of the above experiment was performed with presentations made in the tactile rather than in the visual mode. Raised figures were formed by driving small brads into wood cubes and leaving the heads projecting slightly above the surface. The observer, blindfolded, held his hands on a frame while presentations were made by pressing the cubes against the lower surface of the first digits. Pairs of unlike symbols were presented for a period of about 3 seconds. After the presentation of a second pair the observer was asked to report which of the symbols in the second pair were like symbols presented in the first pair. The experimental and the control series were mixed. In the control series the same symbol was presented on the same side in the second presentation (Fig. 9). In the experimental series the same symbol was presented on the opposite side. Twenty observers made twelve observations each. The results were entirely negative. There was no significant difference in the number of errors made in each series or in the kind of change or orientation which provoked errors. If this arrangement by controlling the movement of the hands might be said to control the use of gestural cues it may be argued that the elimination of such cues makes the misjudgment of inversions and rotations as common as the misjudgment of reversals.

A fourth kind of evidence bearing upon the question of gestural responses in perception was obtained by devising more complex symbols which would be difficult to interpret in terms of gestures. 24 such symbols were devised (Fig. 10). The observers, in groups of approximately 10 were seated 3 to 3½ meters in front of a screen. Before each presentation was made the observers were asked to fixate a small red disk in the center of the screen. They were asked to keep this fixation during the presentation. The presentation was made by the experimenter who stood behind the screen and held two cards, one at either side of the screen, for a period of about 1 second. The cards bore symbols 6 cm. high. A learning series of 12 pairs of symbols and a recognition series of 12 pairs of symbols

were presented. In the recognition series 6 symbols were presented on the same side and in the same orientation, 6 on the opposite side but in the same orientation, 6 on the same side but in reverse orientation, and 6 on the opposite side but in the reverse orientation. The symbols in the learning and recognition series were rearranged several times during the experiment so as to eliminate the effect of varying complexities. There were 135 observers, all undergraduate students with training in experimental psychology. The observers reported that it was impossible consciously to use gestural responses. The results were negative. In the recognition series there was no more tendency to misjudge the reversed symbols appearing on the opposite side than to misjudge the same symbols appearing on the opposite side. There was a slight but statistically insignificant tendency for symbols appearing on the opposite side to be misjudged more frequently than symbols appearing on the same side. Here again we have evidence that gestural responses of the adductor-abductor type are not always used in the perception of symbols and that when they are not so used reversals are as easy to detect as are the other changes in orientation.

The evidence cited has already thrown light upon the third question; what kind of a change in the orientation of symbols is most likely to cause confusion? We answer the question by saying that when gestural responses are discouraged inversions are most confusing but when the presentations are made through the tactile mode one kind of a change in orientation is as difficult to detect as another.

From the experiments we venture to make the following tentative conclusions: (1) Physiological symmetry is a significant factor in the perception of symbols only when gestural cues are used. (2) Inversion is peculiarly difficult to detect in the visual mode. (3) There is no known check on cerebral dominance which is sufficiently dependable to enable one to investigate the influence of that factor on the perception of the orientation of symbols.

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CONTRAST EFFECTS IN JUDGMENTS OF AUDITORY INTENSITIES [1]

BY J. GARTON NEEDHAM [2]

The Method of Single Stimuli,[3] wherein an observer makes absolute judgments upon a series of stimuli presented singly, provides experimental data which involve relative proportions of judgments entirely comparable to those which, in the comparison judgment of successive stimuli, are known as time-errors. This correspondence has been recognized in two ways: (1) theories based upon the time-error have been invoked to account for the appearance of such relationships in the absolute method; [4] (2) the method of single stimuli has been employed in the investigation of time-error phenomena.[5] The significance of the correspondence and the implications which it bears, especially with respect to the temporal formulation of the time-error phenomena, have been discussed previously.[6]

One study in which the method of single stimuli was employed in the determination of certain characteristics of the time-error is that of Pratt.[7] In this investigation it was found that a negative time-error (relative preponderance of judgments 'Loud') appeared in a group of judgments upon a series of auditory intensities, but that with a new stimulus series of considerably less intensity there appeared a positive time-

[1] From the Princeton Psychological Laboratory.

[2] National Research Council Fellow.

[3] The terminology is that suggested by Volkmann, J.: The method of single stimuli, *Amer. Jour. Psychol.*, 1932, 44, 808 f.

[4] Wever, E. G. and Zener, K. E.: The method of absolute judgments in psychophysics, *Psychol. Rev.*, 1928, 35, 466–493; Fernberger, S. W.: On absolute and relative judgments in lifted weight experiments, *Amer. Jour. Psychol.*, 1931, 43, 56–578.

[5] Pratt, C. C.: Time-errors in the method of single stimuli, Jour. Exper. Psychol., 1933, 16, 798–814.

[6] Needham, J. G.: The time-error in comparison judgments: a historical background, *Psychol. Bull.*, 1934, 31, 229–243.

[7] *Loc. cit.*

error (preponderance of judgments 'Weak'). The results were presented as evidence that phenomenal intensities are partially determined by and altered in the direction opposite to the level of reference established by traces of past stimuli which form a homogeneous group with the present stimuli; the weaker stimulus series, in contrast with the louder, was assigned a relatively larger number of 'Weak' judgments. The appearance of a negative time-error in the original series was cited as indicative that the series as a whole was 'Louder' than the sum total of the observers' previous auditory experience.

Pratt's results are supplemented by those of Woodrow,[8] who found a regular variation in the time-error as the absolute level of a pair of comparison weights was changed. Woodrow's experiment differed from that of Pratt in two respects: the judgments were comparative rather than absolute, so that the effect of the standard stimulus must be taken into consideration, and Woodrow's various absolute levels of weight intensity were varied haphazardly throughout a single experimental series, whereas Pratt first obtained all the judgments upon the more intense stimulus series and then presented the weaker series. Thus, the effect of the less intense upon the more intense series was not observed. The present experiment was designed to demonstrate not only the influence of a louder upon a weaker series of stimuli, but also the opposite influence.

The experimental procedure was as follows: the stimuli were auditory intensities, produced by a vacuum tube oscillator circuit, the current from which was led through an attenuator into an ear-phone at the right ear of the observer.[9] The tones were of a constant frequency of 1100 d.v./sec. The duration of the single tones was one second; the interval between successive tones was 6.5 seconds (a rate of 8 per minute).

For the first part of the experiment three stimulus series were selected, at intensive levels which were 20, 40 and 60 db above an approximated *RL*. These series will be referred to as *A*, *B*, and *C*, with *A* the loudest and *C* the weakest level. These settings gave sufficiently extreme intensive levels, and also a series of intermediate loudness. At each level a series of three stimuli was selected in steps which were quickly deter-

[8] Woodrow, H.: Weight discrimination with a varying standard, *Amer. Jour. Psychol.*, 1933, **45**, 391–416.

[9] If a more complete description of the apparatus is desired, it may be found in Needham, J. G., Rate of presentation in the method of single stimuli, *Amer. Jour. Psychol.*, 1935, **47**, to be published.

mined as giving comparable distributions of judgments at the different levels; these intervals were of 2 db at the 20 db level, and of 1 db [10] at the other levels.

At each experimental session the observers made a total of 25 judgments upon each of the stimuli in one of the series; after a rest period of approximately 4 minutes the same number of judgments was made for another series, and after another intermission the third series was judged. The three series were arranged to occur in varying order from one session to the next. In a total of six experimental hours (approximately three days apart) each observer was presented twice with each series in a given position.

The method of single stimuli makes possible the evasion of the involved controversy concerning the use of an 'Equal' or 'Intermediate' category of judgment; the following instructions proved perfectly satisfactory and efficient:

"You are to be presented with single tones. The tones will vary in intensity or loudness, some being more and others less intense or loud.

"Evaluate each tone with respect to its loudness and judge it as loud or weak. Employ the following numerical categories of report:

"'One'—Weak (or soft)

"'Two'—Loud (or strong)."

In the first part of the experiment the two observers were Messrs. F. W. Taylor and S. W. Cummings, graduate students in psychology. Neither was experienced in psychophysical judgments of auditory intensities.

In interpreting the results the following arbitrary assumption was made: the judgments upon a given series (*A*, *B*, or *C*) when that series was the first to be judged in an experimental session were assumed to represent the 'norm' for that series, while the judgments upon the same series when it succeeded another were assumed to represent, by comparison with the 'norm,' the effect upon it of the preceding series. The data which are presented in Table 1 involve this assumption. The values are those for the *D* percent, which is derived from the formula $D = \frac{W - L}{N}$, which indicates the relative preponderance of judgments 'Weak' (when *D* percent is positive) or of judgments 'Loud' (when *D* percent is negative).

Table 1 suggests that the displacement of the time-error as a function of the intensive level of the stimulus series is a bipartite effect. A comparison of the norms for the three series indicates a regular shift in the negative-positive direction as the intensive level is displaced in the louder-weaker (*A* to *C*)

[10] The intervals varied logarithmically, since the intensive relationships, in terms of decibels, of the current actuating the diaphragm of the ear-phone is given by the formula:

$$N(\text{db}) = 20 \log_{10} \frac{I_1}{I_2}$$

TABLE 1
VALUES OF D PERCENT FOR STIMULUS SERIES AT THREE INTENSIVE LEVELS

Ob.	Series *A*			Series *B*			Series *C*		
	Norm	After *B*	After *C*	Norm	After *A*	After *C*	Norm	After *A*	After *B*
Ta.....	−17	−20	−32	−11	−5	−23	+ 2	+20	+13
Cu.....	0	−8	−20	+16	+26	−5	+17	+25	+18

direction. At the same time these differences are magnified or exaggerated to a marked extent when the norm for a certain series is compared with that series when it has been preceded by a louder or weaker series.

An attempt was made to analyze more thoroughly the nature of the displacements which appear in these data; to this end the frequency distributions of the judgments were plotted for each of the various conditions. Figure 1 shows the distri-

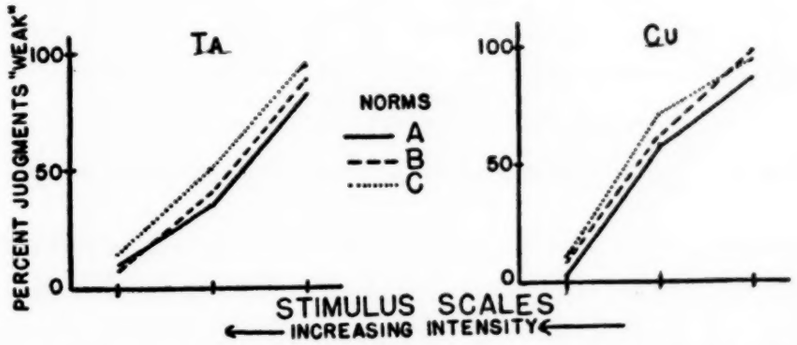

FIG. 1. Frequency distributions of 'Normal' series at three intensive levels.

bution of judgments for each observer upon the three series *A*, *B*, and *C*; the values from which Fig. 1 is plotted are those of the respective 'norms.' The curves are based upon the judgments 'Weak'; since only two categories of judgment were employed the distribution curves for the two categories are complementary.

Figure 1 suggests that in the case of both observers the frequency distributions of judgments for the three 'normal' series undergo a regular displacement from one to the other, and that this displacement tends to be carried by the distributions as a whole. For example, the values of *D* percent for series *A* and *C* indicate a change from a negative to a positive

time-error; Fig. 1 indicates that the introduction of a larger number of judgments 'Weak' which is involved is distributed rather evenly among the three stimuli of which the series are composed. On the other hand, a comparison of the distribution curve for the 'norm' of a certain series with that for the same series when preceded by the judgment of a series at a different intensive level indicated that the displacements in the *D* percent which resulted involve a characteristically different distribution of the displacement among the stimuli of which the series is composed. Fig. 2 shows typical examples of this effect.

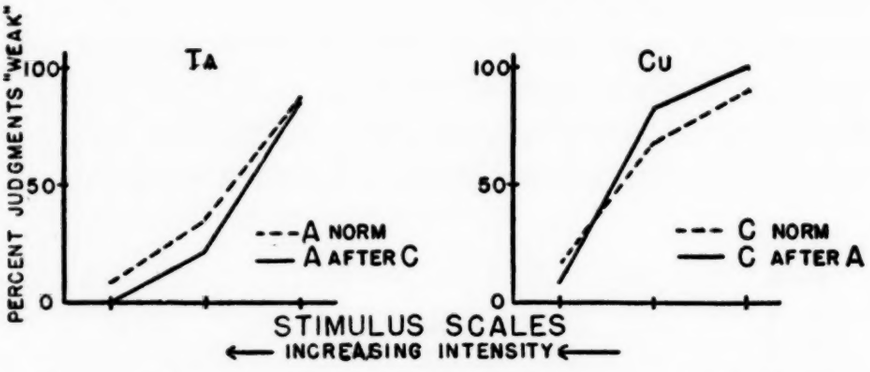

FIG. 2. Typical 'Normal' and 'Contrast' frequency distributions.

However, the inclusion within a single experimental situation of three different intensive levels appeared to be confusing to the observer. It seemed advisable to make a further test of the possibility of a localized effect of contrast; consequently the discussion of Fig. 2 is postponed until the data from the second part of the experiment are analyzed.

Certain new conditions were imposed upon the further investigation of the reciprocal effects of different intensive levels. Two (rather than three) levels of intensity were considered sufficient; the two levels which were selected were respectively 25 and 50 db above an approximated *RL*. These will be referred to as *S* (the louder) and *T* (the weaker level). Secondly, the stimulus ranges comprising the two series were increased from three to four stimuli, in order that the curves for the frequency distributions might be more accurately indicative of any possible displacement. Thirdly, rather than make necessary an involved analysis of the relationships of the

norms, the scale values for the two series were selected in such a manner that the differences between the single stimuli would be more easily discerned in one series than in the other. The use of stimulus steps of 1 db in both series accomplished this purpose.

In every other respect the experimental conditions were the same as in the first part of the experiment. The two observers were Ta (from the first part of the experiment) and Ne (the writer).[11] Ne served with knowledge of the problem and of its conditions; however, the results did not reflect any possible prejudice. At each experimental session the observers made a total of twenty judgments upon each of the four stimuli in a series. A rest period of about five minutes was interposed between the two series. The order of presentation of the two series was varied from one session to the next, as follows:

Day	Ta	Ne
1	*S T*	*T S*
2	*T S*	*S T*
3	*S T*	*T S*
4	*T S*	*S T*
5	*S T S*	*T S*
6	*T S T*	*S T*
7		*T S T*
8		*S T S*

Thus for Ta sixty, and for Ne eighty series of judgments were available for the determination of the two norms, and for comparison with these results there were respectively eighty and one hundred series of judgments upon each stimulus of a series when that series had been preceded by the other. The values of the various determinations of the *D* percent are presented in Table 2. It will be noted that the same relative displacements are present in Table 2 as were found in the first experiment, both with respect to the normal values and to the values for *S* after *T* and *T* after *S*.

The two parts of the experiment are consistent in the find-

[11] The writer wishes to express his gratitude to Mrs. E. B. Needham, who served as experimenter during this part of the experiment.

TABLE 2

Obs.	Loud Series		Weak Series	
	S Norm	*S* after *T*	*T* Norm	*T* after *S*
Ta.....	0	−6	+2	+12
Ne.....	+0.5	−13	+5	+15

ing that the normal determinations of the values of the *D* percent at different intensive levels have the following relationships: judgments upon the weaker series result in a preponderance of judgments 'Weak,' as is indicated by a positive *D* percent. For the louder series the *D* percent is either negative or less positive. This 'normal' difference between the two (or three) series is increased when one of the series is presented to the observer just after he has made a series of judgments at a different intensive level.

However, it may be noted that the differences for both 'normal' and 'contrast' comparisons tend to be considerably greater in the first part of the experiment (Table 1) than in the second part (Table 2). It will be recalled that in the first part the observers had had no previous experience in this type of experiment. The observers in the second part of the experiment might be assumed to be somewhat more adjusted both to the nature of the stimuli and to the method.

It is possible to analyze the data of the second part of the experiment for the purpose of discovering any diminution of the contrast effect which might have occurred. The absolute magnitude of this contrast effect may be derived from the algebraic difference of the values of the *D* percent for two successive series of judgments. For those cases in which an experimental session involved a 'normal' and a 'contrast' series, the *D* percent difference for these two is taken; where a 'normal' and two 'contrast' series were judged, there are two values of this algebraic difference. The sign of the difference will depend upon the direction of the contrast effect; the present concern is solely with its absolute amount. The successive values of this algebraic difference are presented in Table 3. It is immediately apparent that there is a progres-

TABLE 3

	Experimental Session							
Obs.	1	2	3	4	5	6	7	8
Ta.....	19.6	12.0	14.0	11.2	8.4 2.8	5.6 2.8		
Ne....	28.0	16.8	11.2	14.0	14.0	8.4	5.6 5.6	2.8 5.6

sive decrease in the magnitude of the displacement of judgments which is effected by the contrast situation. Indeed, an entirely similar tendency seemed to appear in the data of the first part of the experiment, although it was somewhat masked by the general complexity of the experimental situation.

A further evaluation of the contrast effect is made possible by Figs. 3 and 4, which follow Fig. 2 in presenting for both of

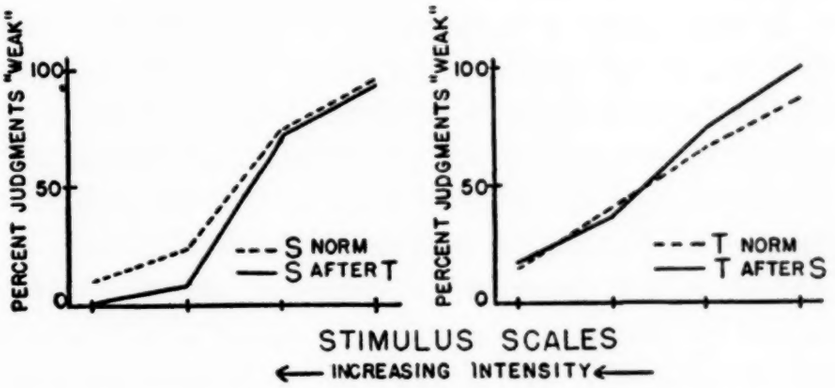

FIG. 3. 'Normal' and 'Contrast' distributions. Observer: *Ta*.

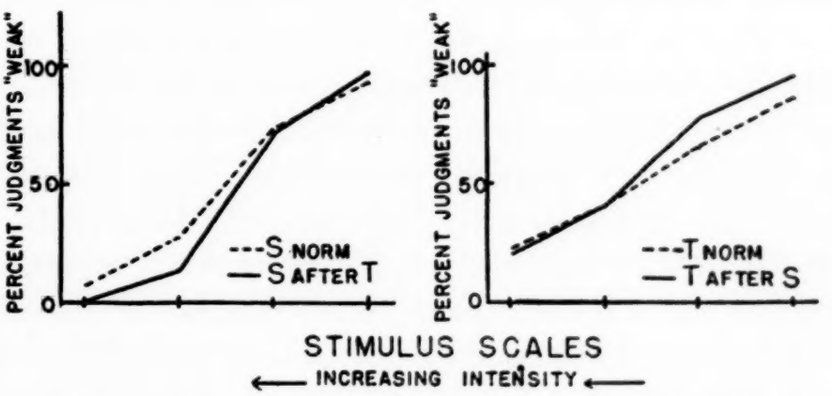

FIG. 4. 'Normal' and 'Contrast' distributions. Observer: *Ne*.

the stimulus series in the second part of the experiment the distributions of judgments for the so-called 'normal' series and

for the same series when it was preceded by a 'contrast' series. In Figs. 3 and 4 the characteristic features suggested in Fig. 2 are preserved and clarified; without making use of any statistical estimate of the significance of the various differences,[12] it seems possible to make the following generalization: the contrast effect which operates when a series of stimuli (auditory intensities) is preceded immediately by another series of significantly different intensity is most (if not entirely) effective in that region of the stimulus scale which is most remote from the intensive level of the preceding series. For example, when Series *T* is judged after Series *S* an increase in the number of judgments 'Weak' upon that series results in a value of *D* percent which is more positive than the 'normal' *D* percent for Series *T*. This increase in judgments 'Weak' is not distributed evenly among the four stimuli which comprise Series *T*, but rather it is localized in that range of the series which is made up of the objectively weaker stimuli. The illustration is reversed for the effect of Series *T* upon Series *S*.

If space permitted it would be possible to demonstrate that this generalization is valid not only for the total distributions of judgments, but also for individual determinations in which a contrast effect appears. Moreover, as the experiment progressed to a point where the contrast effect became less marked or negligible (see above), the graphical analysis of the fractionated data indicated that under 'normal' and 'contrast' conditions the frequency distributions are substantially the same. That is, when Series *S* preceded by Series *T* is unaffected by a contrast effect (as revealed in the values of the *D* percent), the distribution of judgments is the same as that for Series *S* under the 'normal' condition. This statement implies one further aspect of the fractionated data; it was noted that in the latter stages of the experiment the *D* percent values for a given series were approximately the same whether that series appeared in the 'normal' or in the 'contrast' situation.

It is interesting to note that studies of affective contrast

[12] Two methods which might be applicable are the determination of the probable error of a difference from the sigma values of the different observed frequencies, and a factorial analysis. The assumptions of normality which are involved in the one or the other procedure have not yet been reconciled to the present data.

provide data which are remarkedly comparable to those of the present experiment. The laws of affective contrast, as formulated by Bacon, Rood, and Washburn,[13] and by Beebe-Center,[14] might be paraphrased to fit the present results. The fact that in the case of auditory intensities there is a marked decrease of the contrast effect with practice or repetition might very probably be attributed to significant differences in the nature of the judgment. In any case, affective experience is generally considered to be far removed from the more simple auditory intensive experience, and the mere fact that certain striking resemblances do appear suggests the operation of some common factor or factors.

The preceding discussion has emphasized the analytic rather than the generalized treatment of a series of judgments upon a stimulus scale. This approach resulted in a generalization which involved the assumption of a displacement which might involve a certain part of the stimulus series without having any effect upon another part. A third brief experiment was designed to investigate the limiting case of such a displacement. The experimental procedure was as follows: at an intermediate intensive level (40 db above an approximated *RL*) a stimulus scale was constructed in steps of 1 db. This series was composed of three stimuli, which are designated *b*, *c*, *d*, in order of decreasing intensity. The observer made absolute judgments upon this series, the three stimuli being presented in haphazard order until 10 judgments had been made upon each of the stimuli. The scale was then extended by the inclusion in the series of a fourth stimulus, *a*, which was more intense than *b* by 1 db. This *a–d* series was then presented until the observer had made a total of 15 judgments upon each stimulus. The extension was also made in the opposite direction, the *b–d* thus becoming a *b–e* series. The transition from one series to another was always immediate; the first stimulus of the *a–d* (or *b–e*) series followed the last stimulus of the *b–d* series without interruption. In no case

[13] Bacon, M. N., Rood, E. A., and Washburn, M. F.: A study of affective contrast, *Amer. Jour. Psychol.*, 1914, 25, 290–293.

[14] Beebe-Center, J. G.: The law of affective equilibrium, *Amer. Jour. Psychol.*, 1929, 41, 54–68.

was the observer warned or in any way led to suspect that any alteration was taking place in the stimulus series. One observer (We) reported a suspicion of some sort of change upon one occasion; at other times he voluntarily observed that there seemed to be no change. The other observer (Cu) asserted that he was at no time aware of any displacement.

At each experimental session the observer was presented with the *b–d* to *b–e* (or *a–d*) extension; after a rest period the *b–d* to *a–d* (or *b–e*) series was given. The order of the two conditions was altered from one experimental session to the next.

Both of the observers, Cu (above) and We (Dr. C. H. Wedell, Instructor in Psychology) had just completed experiments in which they made absolute judgments of auditory intensities. We made a total of 80 judgments upon each of the stimuli in the *b–d* series, and 60 series of judgments upon both the *a–d* and *b–e* series. Cu made 60 and 45 series of judgments respectively.

This experimental situation provides a case wherein the observer is not aware of any intensive displacement of the stimulus series; the 'universe' within which he is distributing his judgments is objectively different, but unchanged subjectively. Fig. 5 shows the results of this experiment. As

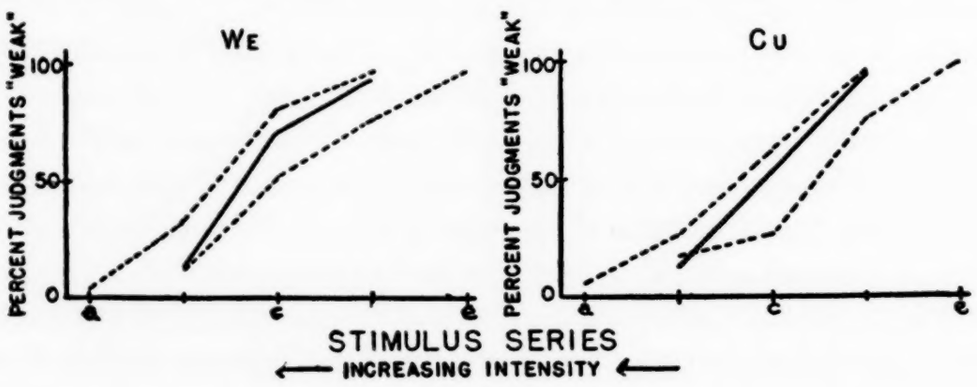

FIG. 5.

might be expected, the introduction of a louder stimulus (*a*) results in a larger number of judgments 'Weak' upon the *b–d* range of the series; this involves nothing more than the tendency of the observer to approximate at all times (when the conditions are not distorted) a relatively consistent distribu-

tion of judgments. When the upper limit of the series becomes *a* rather than *b* the largest number of judgments 'Loud' will be assigned to *a*, and if the consistency of the observer is to be maintained there will be a relatively larger number of judgments 'Weak' upon the other members of the series. However, as was the case in Figs. 3 and 4, this shift in the number of judgments upon the *b–d* series is strictly localized. With the introduction of a new stimulus at the 'Loud' end of the scale there is no significant change in the proportion of judgments at the 'Weak' end, and vice versa.

Figure 5 lends credence to the possibility which was suggested in the two previous parts of the experiment, namely, the assumption that certain displacements of the asymmetry of judgments upon a given series of stimuli may be attributed entirely to certain members or a certain range of that series. The specific locus within the series of this displacement would then become a question of the conditions which cause the shift.

On the methodological side, this suggestion seems to warrant a departure from the usual hypothesis that the absolute judgment is mediated by a mental 'standard' or series midpoint. It also raises some question as to the precise definition and applicability, under a diversity of conditions, of the concepts of a central tendency of judgment and of an indifference point. The absolute judgment is discussed elsewhere [15] in terms of an arbitrary 'universe' with which the observer is presented under various conditions, and within which he is requested to make the reports appropriate to the instructions under which he is serving. There does not appear to exist any principle which will provide an appropriate representation of the performance of the observer under all kinds of conditions. For example, it has been found that in the Method of Single Stimuli the displacement in the *D* percent which is caused by a change in the rate of presentation is carried entirely by the intermediate members of the stimulus series.[16] In the present experiment there are instances where the displacement seems

[15] Needham, J. G.: Rate of presentation in the method of single stimuli, *Amer. Jour. Psychol.*, 1935, 47, to be published.

[16] *Ibid.*

to be mediated by the series as a whole (Fig. 1), and other cases in which the shift is carried at the one or the other extreme of the stimulus series. Again, the discussion of the effect of repetition was concerned with two discrete 'universes' which were first intereffective, later independent.

A series of studies of which this is one is attempting to investigate the typical features of the judgment or report of the observer in various situations; this judgment is always a function of the 'universe' which is created for the observer, of the various conditions which affect this universe, and of numerous factors attributable to the observer himself. The elucidation of the effects of the various determinants of a judgment *per se* would lead not only to a clearer understanding of what is most assuredly a 'higher mental process,' but also would be significant with respect to the whole field of 'discrimination.'

Summary

1. Contrast effects appear in absolute judgments of auditory intensities when the judgment of a series of stimuli is preceded by the judgment of a series at a significantly different level of intensity.

2. The nature of these effects is an increase or a decrease in the relative preponderance of one or the other category of judgment.

3. There is some indication that with practice the contrast effects are diminished or disappear altogether.

4. The direct contrast effect is mediated, not by a series of stimuli as a whole, but by the judgments upon certain definite members of the stimulus scale.

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THE BELONGINGNESS OF NON-BELONGING IMPRESSIONS

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In his recent book entitled "Human Learning," Thorndike discusses his laws of Exercise and Effect at great length, and proposes a new principle, the so-called 'Principle of Belonging,' as fundamental to these laws of learning.[1] The last-mentioned fact has given rise, in the writer's mind, to the following questions: Is the Principle of Belonging one of universal validity? And, if so, is it the ultimate principle of learning? This paper is intended to offer an answer to these questions.

I. A Conceptual Analysis of the Principle of Belonging

In discussing the Principle of Belonging, Thorndike says, "If a man experiences *A* and *B* in succession repeatedly without any sense that *B* after *A* is right and proper, or even that *B* belongs with *A*, and without himself producing *B* when he suffers *A*, the influence upon the man is very, very slight. You practically always raise the body and bend it back after tying your shoes, and so have the sensations of bending the body back as a sequent to those of tying your shoes. You have done this from say 10,000 to 40,000 times (according to your respective ages and predilections about changing your shoes often), but the experience of tying your shoes has probably never called to mind any sensation, image, or idea of the backward body-bend in one person in 1,000. Here sequence with no fitness or belonging has done little or nothing.

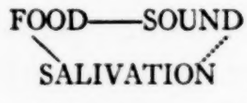
[1] The writer, in his recent publication entitled 'Analyses of Laws of Learning' (which is written in Chinese), has subjected these laws of learning to an experimental analysis, and also casually touched upon the Principle of Belonging. Portions of this book will be translated into English in the near future.

"It is hard to determine experimentally just how little it does since repeated connecting of any sort with very little attention on the part of the person to what is going on also does very little. And mere temporal contiguity without any sense of belonging or acceptance of the sequence as right or proper often implies inattentiveness or a very low degree of attention to the sequence in question. We need to test mere temporal contiguity, with full or at least average attentiveness." [2]

The gist of these statements amounts to this: In case that there are two experiences *A* and *B* which occur in succession but which do not belong together, this sequence, however often it repeats itself, will not bring about a connection between these experiences. But here the question arises: What is meant by the term 'belongingness'?

It has been found by Pavlov in his experiments with dogs that, after occurring simultaneously with the presentation of food a number of times, the sound of a buzzer alone is capable of causing an increase of salivation in the animal. With regard to the relationship of this finding to the psychology of learning, Thorndike has the following criticism: "The picture which it presents differs from that which we have found in our experiments in several important respects. First, mere sequence and contemporaneity with little or no belonging causes the learning. The food may belong physiologically with the sound, and the increase in the flow of saliva may belong physiologically with the food. But the increase in the flow of saliva can hardly be said to belong with the sound; it simply follows it and goes along with it." [3]

The relationships here discussed may be graphically represented as follows:



The solid line denotes belongingness.

The dotted line denotes non-belongingness.

In looking at the above graph, we may note that food and sound constitute a situation, whereas salivation represents a response. The fact that the components of a situation belong

[2] Thorndike, E. L., *Human Learning*, The Century Co., N. Y., 1931, pp. 18–19.

[3] *Ibid.*, p. 106.

together does not insure their belongingness with a common response. A response must be originally or naturally related to a situation in order to belong to it. Here an original or natural relationship is looked upon as the sole criterion of belongingness.

Thorndike has performed a number of experiments to test the influence of temporal contiguity. In one of these experiments, the material used consists of a long list of word-number pairs. In the list, each word is followed by a definite number, and certain numbers are also followed by definite words. These pairs appear with various frequencies. The experimental procedure is to read the list aloud to the subjects, and then to ask them to write out the numbers that follow certain words and the words that follow certain numbers. Results show that pairs of the former type are remembered but not those of the latter type. This fact is regarded by Thorndike as corroborating the Principle of Belonging, for in this case, the connections formed between words and numbers are governed by their belongingness. The nature of the instruction, the way the list is read and general habits of life—all these have contributed towards the belief of the subjects that words belong to the numbers that follow them but numbers do not belong to the words that come next. The latter sequence involves mere temporal contiguity, and lacks belongingness; and therefore its repeated occurrence fails to make its influence felt. Here the criterion of belongingness seems to be found in the relationships that are acquired.

This analysis reveals the following facts: A situation and a response must belong together in order that a connection may be formed between them. The connections among the components of a situation are also governed by their belongingness. Mere sequence will not bring forth a connection. As to the nature of belongingness, it remains a question as to whether it is natural or acquired, for Thorndike himself fails to be consistent on this point.

Experimental Studies of the Principle of Belonging

In accordance with the Principle of Belonging, two impressions must belong together in order that they may be con-

nected with each other; mere sequence will not cause them to be connected. However, attention should be drawn to the fact that, in Thorndike's experimental list, all the impressions form pairs, or, in Thorndike's terminology, every impression is to belong somewhere. But, if there are in the list impressions that belong nowhere, we should like to know whether these impressions can also form connections with others. This is the problem we have attempted to solve.

With a view to solving this problem, we have performed a number of experiments. Some of these experiments will be reported elsewhere in detail, and therefore a brief account of them will suffice in this paper. The material used in these experiments consists of both character-number lists and number-character lists. In the former, each pair of characters [4] is followed by a pair of numerals, while, in the latter, each pair of numerals is followed by a pair of characters. Each pair is written on a horizontal line, but these pairs form a vertical series. In each list there are ten character-number or number-character pairs. The experimental procedure was to instruct the subjects to connect each character-pair in the character-number list with the number-pair that followed it, and to connect each number-pair in the number-character list with the character-pair that followed it. The lists were presented by means of an exposure apparatus. One pair was exposed at a time, and the rate of presentation was two seconds for each pair. After the twentieth presentation of each list, the subjects were tested by recalling the pair of impressions on the presentation of that with which it was to be connected. They were also required to recall the impressions that did not so belong.

Four groups of subjects were used. These may be designated as *A*, *B*, *C* and *D* respectively. There were thirty subjects in each group. What is of interest here pertains only to Groups *B* and *C*. The material used in Group *B* is characterized by the addition of a pair of characters at the head of the number-character list and by the addition of a pair of numerals at the head of the character-number list. The average percentage of number-character pairs remembered in the former list and that of character-number pairs remembered in the latter list are 28 and 64.7 respectively, but there are, in both cases, 83 percent of the subjects knowing the relationship of the non-belonging pair to that which follows it.

The material used in Group *C* is characterized by the addition of a pair of numerals at the end of the number-character list and by the addition of a pair of characters at the end of the

[4] These are intended to serve the same purpose as nonsense syllables. Every Chinese character has a meaning, but a pair of characters can be so chosen as to give no meaning.

character-number list. When tested 33 percent of the subjects knew what preceded the last pair of numerals, and 57 percent of them knew what preceded the last pair of characters.

These results clearly show that the non-belonging impressions can also be retained, and that they also form connections with the impressions that are adjacent to them. Such findings, however, are capable of several interpretations, which will be considered separately.

I. These findings may be interpreted to mean that there was a tendency on the part of the subjects to connect characters of the number-character list and numerals of the character-number list with their respective sequents. Were this hypothesis sound, then the process of pairing would start with the first impression in Group *B*, and with the second impression in Group *C*. These formations would be diagonally opposed to the instructions. Moreover, according to the findings with regard to Group *D*, if the subject is free to choose between the number-character and the character-number type of organization, he will almost invariably start with the first impression whether the list is of the number-character or of the character-number sort. Viewed in this light, if it were true that the pairing process was started with the first impression in Group *B*, but not in Group *C*, these divergent tendencies would be unexplainable.

II. There are previous studies indicative of the predominance of the first and the last impression. This fact might be regarded as affording an explanatory basis to our findings. In other words, it might be conjectured that the reason for the retention of the non-belonging impressions in our lists was to be found in the favorable positions these impressions occupied. But this explanation involves two problems, which should be distinguished each from the other. One of the problems is: Are the impressions in the first and last positions of a list easier to retain than those in the other positions of the list? The other problem is: Is it necessary that the non-belonging impressions occupy the first or the last position in the list in order that they may form connections

with other impressions? The first problem pertains to the relative impressiveness of different positions of a list, and therefore it is not the problem with which we are here concerned. It is the second problem that is now confronting us, and the experiment reported below is intended to furnish a solution of the problem.

This experiment differs from the previous ones only in one respect, namely that the non-belonging pair is located in the middle of the list. There were fifty-two subjects, who were all second-year students of a girls' high school. The findings are as follows:

The average percentage of number-character pairs remembered is 87.7. 76.92 percent of the subjects knew the relationship of the non-belonging pair to the preceding one, and 57.69 percent of them knew its relationship to the following one. The average percentage of character-number pairs remembered is 86.2. 88.46 percent of the subjects knew what preceded the non-belonging impression, and 75 percent of them knew what followed it. These results suffice to show that the connection formed between the non-belonging pair and others is not due to the favorable position it occupies, for its location in the middle of the list does not prevent it from being connected with the pairs that are adjacent to it.

III. We have shown above that, whether located at the head, at the end or in the middle of a list, the non-belonging impression tends to connect itself with other impressions. This may be due to the fear, on the part of the subjects, of being questioned concerning the non-belonging pair. Such an explanation certainly sounds plausible, but its validity seems to be doubtful in the light of the following experiment:

In this experiment, two groups of subjects were used: the first group were forty-one sixth-year pupils of a primary school, and the second group were thirty-three college students. The material used for the two groups was exactly the same. It comprised two lists which may be designated as "*A*" and "*B*" respectively. List *A* was composed of ten character-pairs, while list *B* was headed by a pair of characters, the rest being five character-pairs and five number-pairs alternately arranged. The material was presented by means of an exposure apparatus, and each pair was exposed for two seconds. The test was given after the sixth presentation of each list.

The instruction for List *A* was as follows: "I will ask you to read a list, in which there are many pairs of characters. After reading it several times, you will be required

to write out the pair that precedes each pair. (Illustration.) Attention! In learning the list, you should start with the second pair, for it is not necessary to attend to the first pair. Ask questions if you do not understand."

The instruction for List *B* was as follows: "I will ask you to read a list, in which there are some character-pairs and some number-pairs. These are alternately arranged. After reading the list several times, you will be required to write out the number-pair that follows each character-pair. (Illustration.) Attention! In learning the list, you should start with the second pair, for it is not necessary to attend to the first pair. Ask questions if you do not understand."

The results of this experiment may be summarized as follows: The average percentage of character-pairs remembered by the first group in List *A* was 42.75, but 76 percent of the subjects knew what came after the first pair of this list. The average percentage of character-number pairs remembered in List *B* was 80.4, but 80 percent of the subjects knew by what the first pair of this list was followed. With regard to the second group, the average percentages of achievement in Lists *A* and *B* were 46.25 and 83.60 respectively; but 69.69 percent of the subjects knew the sequent of the first pair of the former list, and 48.48 percent of them remembered that of the latter list. Considering the negative aspect of the results, we do find that a certain number of the subjects failed to learn the relationship between the first pair and the next one. But this fact is not necessarily attributable to the non-belongingness of the first impression, for the subjects also failed to recall some of the impressions that were to belong together. But, in view of the positive aspect of the results, the following conclusion is justified: if the subjects looked upon the first pair as a non-belonging impression, and yet knew its relationships, then it can not be said that the non-belonging impressions are incapable of forming connections with others. In this experiment, as the result of several conditions, the first pair necessarily became a non-belonging impression in Thorndike's sense; but, in spite of that, it was capable of forming a connection with the following pair. This fact seems to be a sufficient refutation of the Principle of Belonging.

IV. There is, however, another possible interpretation of the findings reported above, namely that, the connection formed between the non-belonging impression and the following one may be due to the similarity of their constitution. To

test this hypothesis, we have performed the following experiment:

There are four lists used as material for this experiment, which may be designated by the letters '*M*,' '*N*,' '*O*' and '*P*' respectively. List *M* contains eight pairs of numerals with a pair of characters inserted in the middle of the list. List *N* contains eight pairs of characters with a pair of numerals inserted in the middle of the list. List *O* is similar to List *N* except that the same pair of numerals is located at the head of the list. List *P* is similar to list *M* except that the same pair of characters is located at the head of the list.

Two groups of subjects were used: the first group of fifty-four and the second group of thirty-eight subjects. They were all fifth year pupils of a primary school. Lists *M* and *N* were to be learned by the first group, and Lists *O* and *P* by the second group. The lists were presented by means of an exposure apparatus, and the time of exposure was two seconds for each pair. Before the presentation of each list, the following instruction was given:

The Instruction for List *M*: "I will ask you to read a list, in which there are several pairs of numbers. After reading the list several times, you will be asked to write out both the number that precedes and that which follows each number-pair. (Illustration.) You must memorize the list as a whole. Attention! In learning the list, you should not attend to that which is not a number. Your are expected to learn only numbers and nothing else."

The instruction for List *N:* "I will ask you to read a list in which there are several pairs of characters. After reading the list several times, you will be asked to write out both the character-pair that precedes and that which follows each character-pair. (Illustration.) You must memorize the list as a whole. Attention! In learning the list, you should not attend to that which is not a character. Your are expected to learn only characters and nothing else."

The instruction for List *O* is the same as that for List *N*, and the instruction for List *P* is the same as that for List *M*.

Each list was presented six times before the test was given. After finishing the test, the subjects were to answer the following question. Did you memorize the extra pairs because you thought that you would be questioned thereupon?"

The average percentage of number-pairs remembered by the first group in List M is 66.14. Of this group, 85.19 percent succeeded in recalling the character-pair of this list, 37.04 percent knew what followed this pair, and also 37.04 percent knew what preceded it. The average percentage of character-pairs learned by this group in List *N* is 66.14. 83.33 percent of the subjects remembered the number-pair, 44.44 percent of them its sequent and 42.59 percent its precedent.

The second group, on the average, learned 33.07 percent of character-pairs in List *O*. The number-pair of this list was remembered by 81. 58 percent of the group, and its sequent by 76.32 percent of them. Their average achievement is

73.71 percent of the number-pairs. 86.84 percent of them were able to recall the character-pair of this list, and 78.95 percent of them knew by what this pair was followed.

In this experiment, the non-belonging impressions differ in constitution from the others, and therefore can not be looked upon as belonging to the latter by virtue of the constitution. Moreover, the formation of such a relationship was explicitly prevented by the instructions. But, in spite of this, a connection was formed between the non-belonging impression and the others, and hereby the limitation of the Principle of Belonging is clearly exposed. However, it will be of interest to know whether or not the subject's attention to such relationships is due to the fear of being questioned thereupon.

On the basis of answers to the last question, the subjects may be classified as follows: (1) those who were apprehensive of being questioned and answered correctly; (2) those who were apprehensive of being questioned but failed to answer correctly; (3) those who were not apprehensive of being questioned but answered correctly; (4) those who were not apprehensive of being questioned and failed to answer correctly. The results of this analysis are given in Table I. There the terms 'character-pair' and 'number-pair' refer to the non-belonging pairs of such constitution. The term 'sequent' refers to the pair that comes after the non-belonging pair, and the term 'precedent' refers to that which goes before it. This table shows that there is a high percentage of subjects who were not apprehensive of being questioned but who answered correctly, and there is also a high percentage of subjects who were apprehensive of being questioned but who failed to answer correctly. From these facts, we may infer that no necessary relationship exists between the apprehension of being questioned and the memory for the non-belonging impression. In other words, this memory does not owe its origin to the apprehension of being questioned although improvable through such an apprehension.

V. All the findings reported above are consistent in showing the existence of the memory for non-belonging impressions. However, it remains to be determined whether or not this has

TABLE I

THE DISTRIBUTION OF SUBJECTS IN PERCENTAGES

Group	I						II			
List	*M*			*N*			*O*		*P*	
Relationship	Character-pair	*S*	*P*	No.-pair	*S*	*P*	No.-pair	*S*	Character-pair	*S*
Subjects:										
Apprehensive and right	28%	30%	45%	24%	26%	52%	53%	58%	65%	70%
Apprehensive but wrong	9%	8%	40%	20%	17%	32%	18%	24%	14%	16%
Inapprehensive but right	28%	26%	11%	35%	33%	7%	18%	13%	14%	8%
Inapprehensive and wrong	34%	36%	4%	20%	24%	9%	11%	5%	8%	5%

S = Subsequent. *P* = Precedent.

resulted from the fact that the lists were learned as a whole. First of all, attention should be called to the following fact: namely that, if the subjects had actually learned the list as whole (which, for the sake of convenience, will be hereafter designated as the 'total list method'), the Principle of Belonging would also be thereby invalidated; for, with the exception of the experiment reported under IV, the subjects were to regard only certain impressions as belonging to their sequents and not to regard the latter as belonging to those that followed them. Therefore, the adoption of the 'total list method' could mean nothing other than the formation of a connection between non-belonging impressions, and such a behavior-tendency would certainly furnish a contradiction to the Principle of Belonging.

It seems, however, that the question put forth above is still worth considering. We can very well answer this question on the basis of the experiment reported under II. In this experiment, the subjects were required, at the end of the test, to state whether their method of learning was to memorize the list as a whole or only to connect certain impressions with their subsequents. On the basis of these answers, the subjects were classified into two groups: one group composed of those who used the total list method and the other group of those who used the pairing method. If there is found, in the group that used the pairing method, a larger percentage of subjects knowing the relationships of non-belonging impressions than in the group that used the total list method, then the memory for non-belonging impressions will not be entirely attributable to the adoption of the total list method.

This analysis has brought to light the following facts: In the experiment where the number-character list was used, three of the subjects gave no answer to the question concerning the method adopted. Among the rest, there are 59.18 percent using the pairing method, and 40.82 percent using the total list method. These percentages show the preponderance of the subjects who used the pairing method. Among these, 52 percent knew both the precedent and sequent of the non-belonging impression; 10 percent knew its sequent; 31 percent knew

its precedent; and 7 percent were totally ignorant of its relationships. Among those who used the total list method, the corresponding percentages are: 35, 15, 40 and 10. These results clearly indicate that the pairing group contains more of the subjects who knew the relationships of the non-belonging impression than the total list group.

In the experiment where the character-number list was used, there are five subjects who gave no answer to the question on method. Among other subjects, there are 57.45 percent using the pairing method and 42.55 percent using the total list method. In the former group, 67 percent remembered what had appeared before and after the non-belonging impression, 7 percent remembered what had come after it, and 25 percent remembered what had gone before it. Of those who used the total list method, the corresponding percentages are 60, 10 and 20. There are still 10 percent of this group, who were entirely ignorant of the relationships of the non-belonging impression. In this experiment, there is again a higher percentage, in the pairing group, of those who knew the bi-directional relationships of the non-belonging impression.

From these findings, we may infer that the memory for non-belonging impressions is not entirely due to the use of the total list method. But, as this methodological analysis is merely a subsidiary procedure in the experiment, we have performed a number of experiments for the sole purpose of clarifying this point. The procedures and results are reported below:

The material used for this series of experiments consists of two lists, which may be called 'Lists *I* and *J*' respectively. In List *I*, the first pair is of numerals, and the rest are ten pairs of characters and ten pairs of numerals alternately arranged. In List *J*, the last pair is of characters, and the rest are also ten pairs of characters and ten pairs of numerals alternately arranged. The lists were presented by means of an exposure apparatus, and the time of exposure was one second for each pair. List *I* was presented before List *J*. There were twenty-six first-year students of a girls' high school

serving as subjects. They were tested after the tenth presentation of each list.

The instruction given before the presentation of the material was as follows: "I will ask you to read a list, in which there are some character-pairs and some number-pairs. After reading the list several times, you will be required to write out the number-pair that follows each character-pair. Attention! In learning the list, you have to see what comes after each pair of characters. (Illustration.) You should not learn the list as a whole. You should memorize only what comes after each pair of characters." (The last statement was to be emphatically read and repeated.)

The results of this experiment are given in Tables II and III. The numbers in the first column refer to the correctly

TABLE II

RESULTS OBTAINED WITH LIST *I*

Questions	1	2	3	4
Subject				
A	5	1	X	V
B	8	0	X	X
C	6	1	X	V
D	4	1	X	V
E	5	0	X	V
F	4	2	V	X
G	6	1	X	V
H	8	2	V	V
I	4	0	X	V
J	2	2	X	V
K	7	1	O	X
L	9	4	X	X
M	10	4	X	V
N	8	0	X	X
O	8	1	X	V
P	8	0	X	V
Q	6	0	O	X
R	6	2	V	V
S	7	4	X	X
T	4	1	X	V
U	5	0	V	V
V	5	0	X	V
W	7	4	X	V
X	6	0	X	V
Y	1	6	V	V
Z	6	2	X	V
Totals	155	39	V5 X19	V19 X7
Averages	5.96	1.5		
Percentages	59.60%	15%	V19.23%	V73.08%

X = wrong. V = right.

TABLE III
RESULTS OBTAINED WITH LIST *J*

Questions	1	2	3	4
Subject				
A	8	0	V	X
B	3	0	V	X
C	5	0	X	X
D	2	0	X	O
E	7	0	V	O
F	6	3	X	X
G	7	5	V	V
H	8	5	V	V
I	10	1	V	X
J	3	0	O	X
K	6	3	O	X
L	8	2	V	V
M	10	5	X	O
N	10	0	V	X
O	10	7	X	V
P	10	0	V	V
Q	5	0	V	X
R	6	0	X	V
S	9	5	V	V
T	6	2	V	V
U	4	1	V	X
V	5	2	X	X
W	6	0	V	O
X	3	0	V	X
Y	4	0	V	V
Z	9	3	V	V
Totals	170	44	V17 X7	V10 X12
Averages	6.54	1.69		
Percentages	65.38%	18.30%	V65.38%	V38.46%

produced pairs of numerals that followed characters; those in the second column refer to the correctly produced pairs of characters that followed numerals; those in the third column refer to the non-belonging pairs that are correctly recalled; and those in the fourth column refer to the correctly produced impressions that were adjacent to the non-belonging pairs. It may be noted that the results given in the second column pertain to the pairs that were correctly produced on the presentation of the pairs to which they did not belong, for the subjects were required to observe only the sequents of characters. It is true that there is a large number of failures in the second column, but this fact does not lend such a support to the Principle of Belonging as it first seems. The instruction given in this experiment prevented the subjects not only from re-

garding the characters as belonging to the numerals that preceded them, but also from attending even to the succession of their appearance. Under such conditions, even if all the subjects had been failures in the second column, the Principle of Belonging would not be thereby confirmed; for there was lack of ordinary attention even to pure sequence, which is regarded by Thorndike as different from belongingness. But the fact is that there are 38.46 percent and 42.31 percent of subjects who succeeded in recalling at least two such pairs in Lists *I* and *J* respectively, and this fact is in direct contradiction to the Principle of Belonging.

With regard to the impressions that belong nowhere, there are 73.08 percent of the subjects knowing the relationship of the first pair to the following one, and there are 38.46 percent of them knowing the relationship of the last pair to the preceding one. These percentages suffice to show the possibility of the non-belonging pairs forming connections with the other pairs.

All the findings reported above agree in showing that, under the conditions prescribed by these experiments, whether located at the head, at the end or in the middle of a list, whether similar to or different from other impressions in constitution, whether the use of the pairing procedure or the discrimination of the list material is stressed in the instruction, and whether the total list or the pairing method is adopted by the learner, the non-belonging impressions are capable of forming connections with the impressions that are adjacent to them.

Fallacies and Limitations of the Principle of Belonging

In analyzing the Principle of Belonging, we have found that Thorndike's conception of 'belongingness' is inconsistent. This term is intended to denote only the original or natural relationship of things, but it refers sometimes also to the relationship of things that is acquired. The inconsistency has proven to be an obstacle in the application of the principle, and

its effect is clearly evident in Thorndike's criticisms of the conditioning hypothesis and of the Gestalt theory.[5]

Apart from the inconsistency above mentioned, the Principle of Belonging involves certain fallacies and limitations. As we have seen, according to the first criterion of belongingness, a situation and a response must have a natural relationship between them in order that they may be connected with each other. But Thorndike's concept of learning involves the formation of connections as the process and the relationship between a situation and a response as the result. Isn't the above statement of the first criterion equivalent to putting the cart before the horse? Even if this point could be answered by saying that the function of learning lies in the elaboration of the relationship between a situation and a response, attention should be called to the fact that, were the possibility of learning limited to the situations and responses that have a natural relationship between them, then the capacity for learning would be pitiably small![6]

With regard to the concept of the belongingness that is acquired, the limitations of it have been experimentally shown. All the above studies testify to the fact that the things that are to occur in succession but not to belong together are also capable of forming connections with one other. With whatever methods of control and with whatever methods of analysis, the same results have been obtained.

Furthermore, this principle involves other untenable assumptions. It is assumed that two experiences *A* and *B* are capable of mutual association independently of the situation in which they are embedded. The possibility of such an association depends merely upon the recognition, on the part of the learner, of the belongingness of the two experiences. But the fact is that the formation of a connection between *A* and *B* is ultimately conditioned by the situation in which these experiences are embedded. Lewin of Berlin has reported the results of a series of brilliant experiments,[7] which may be cited in

[5] See Thorndike, E. L., *Human Learning.*

[6] This statement is here intended only to mean that impressions are capable of forming connections with each other without necessarily belonging together.

[7] Lewin, K., 'Das Problem der Willensmessung und das Grundgesetz der Assoziation,' I. u. II. *Psychol. Forsch.*, 1922, 1, 191–302; 2, 65–140.

support of this point. The general procedure in these experiments was to ask the subjects to react to each syllable in a definite manner. After repeating the reaction a large number of times, the subjects were required to respond to the same syllable in a different manner. Results are definitely indicative of the fact that if the subjects failed to recall the situation that had existed at the time of learning, then there would be no interference between the learned response and the new one. That is, the learned response would manifest no tendency to reappear. Viewed in this light, the belongingness of *A* and *B* per se does not seem to be such a decisive factor as the situation of their embedding.

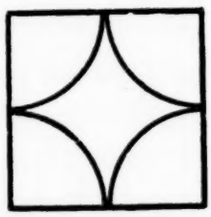
However, it can not be said that Thorndike is entirely ignorant of the importance of this point. In his 'Fundamentals of Learning,' he says "In such experiments, connections are formed whereby 'act' evokes 10, or 120 evokes 25, provided that one thinks of 'act' or 120 in cooperation with a set of the mind toward thinking of some number from 10 to 99, or toward remembering what happened in the experiment, or toward contemplating the problem of connection-forming and data concerning it, or the like. Without some such appropriate contributory set of mind the hearing or seeing of 'act' and 120 has a very much weaker tendency to evoke 10 and 25." [8] At first sight, these statements seem to make up very well what is missing in the Principle of Belonging, but, on the contrary, the limitation of this principle is thereby exposed. The need for a particular set of mind at the time of recall is due to the fact that while a connection was being formed between *A* and *B* or any other two experiences, one was more or less aware of these experiences as belonging to a particular situation. Therefore, it is less true to say that these experiences belong to each other than to say that they belong to a particular situation. And we may go so far as even to say that, if these experiences do not belong to each other at the time of learning, in so far as they belong to one and the same situation, they will be mutually provokable at the time of recall. Plenty of evi-

[8] Thorndike, E. L., *Fundamentals of Human Learning*, Bureau of Pubs., T. C., Columbia Univ., N. Y., 1932, pp. 114–115.

dence for this statement is available in the results of our experiments. But, according to Thorndike, the term 'belongingness' refers to the belongingness of coordinate elements of experience and not to the belongingness of the parts to the whole. As the belongingness of the latter kind is essential to the function of recall, it is doubtless also of importance to the process of learning. Thorndike failed to consider this factor in discussing the process of learning, and therefore the concept of mental set was invoked to explain away the resulting difficulties in the problem of recall.

The Principle of Belonging also involves the assumption that all impressions are primarily independent, and that therefore connections among them are made possible only through a synthesizing process. This, again, presupposes the assumption that the different parts of the nervous system are not functionally related. Both assumptions are contrary to facts as found in the studies of Gestalt psychology and those of electrophysiology.

Attention should be also called to the fact that, even if the things that were to be learned did belong together, and even if the learner is able to recall the situation that existed at the time of learning; it is not necessary that these things are mutually provokable. For instance, the picture below is composed of parts that can be easily recognized as belonging together.



The part will seldom call forth any of the following parts: , even if the situation that existed at the time of learning is simultaneously recalled. This is only one of many cases of this sort.

Let us now conclude by pointing out the logical implica-

tions of the Principle of Belonging. Were the possibility of learning determined by the natural belongingness of things, then the capacity for learning would be pitiably small! Were the possibility of learning determined by the belongingness of things as such, whether natural or acquired, then many cases of learning should be excluded from the psychology of learning! Were the Principle of Belonging the ultimate principle of learning, then the situation at the time of learning (involving the attitude of the learner, the background of things, spacial and temporal factors, etc.) should not play such an important part in the ultimate determination of responses! Were this principle absolutely indispensable to the success of learning, then the different parts of the nervous system should not be functionally interdependent! Were this principle always valid with regard to the things that are to have definite relationships, then exceptions should not be found in such abundance!

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AN ATTEMPT TO CONDITION VARIOUS RESPONSES TO SUBLIMINAL ELECTRICAL STIMULATION *

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Introductory Statement

The influence of subliminal stimuli on conscious and unconscious bodily activities has been experimentally demonstrated in a number of situations. Peirce and Jastrow (7) have shown that judgments of the heavier of two imperceptibly different weights were more often right than wrong although the judgments were made with zero confidence. Dunlap (4) reported an experiment in which physical interruption of inaudible sounds was perceived. The same investigator (5) found that the Muller-Lyer illusion could be elicited with the wings on the ends of the lines invisible. Bressler (1) has shown that the magnitude of this illusion varies directly with the strength of the subliminal visual stimuli. Suslowa (8) has shown that æsthesiometer sensibility was lowered under electrical stimulation even when the current is subliminal. The results of these experimenters bring out the efficacy of subliminal stimuli in judgment, illusion, and sensibility. The question arises as to whether or not stimuli such as these, which are of insufficient strength to produce conscious activity, can be connected with some response by the learning process.

In such learning the subject would not be conscious of the stimulus with which his response was connected, and the learning might suggest the mechanism operative in several functional disorders as well as in the development of certain physiological activities early in life. Moreover, if such learning could be demonstrated, conditioned responses to sub-

* The experiment was carried out under the direction of Professor Hulsey Cason.

liminal visual, auditory, and verbal stimuli in everyday life would be plausible.

Previous experiments have shown that conditioned responses could be established without the subject's cognizance of the response. First proof of such conditioning was established by Cason (**2**, **3**) who conditioned pupillary and eyelid responses to auditory and electric stimuli. Hudgins (**6**) recently confirmed Cason's pupillary experiment and also conditioned the pupillary reaction to voluntary control. However, it has not been proven that conditioned responses could be established without the subject's cognizance of the stimuli. There has been but one previous attempt to condition responses to stimuli below the absolute threshold. In an experiment published in the Jan. 1934 number of this journal (**3**a), Cason and Katcher attempted to condition an increase in breathing, a decrease in breathing, an increase in the frequency of the eyelid response, and a decrease in the frequency of the eyelid response to subliminal electrical stimulation, using six subjects for each of these responses. Their method was to measure the natural effect of the subliminal shock, then to give several repetitions of the shock-response combination followed by a conditioned response test. After a number of shock-response combinations and conditioned response tests, there was a period of adaptation in which the subliminal shock remained on for fifteen minutes. Finally the effect of the subliminal shock on the response was measured. The results were negative on all four responses, but a number of suggestions for further experimentation were offered, and several of these have been followed in the present experiment.

We have attempted to condition (*a*) Increase in breathing, (*b*) Increase in the frequency of the eyelid response, and (*c*) Increase in foot pressure to subliminal electrical stimulation. The general method used was as follows. At a telegraphic signal, *S* voluntarily increased the response to be conditioned, and at a second signal he voluntarily resumed normal activity in the response. During the period of the increased response either a supraliminal or subliminal shock was on. After a large number of repetitions of these shock-response or *training*

combinations, the response was measured during alternate periods with the shock on and off, and all of these shocks were subliminal, this being the *conditioned response test.* In this way we determined whether or not subliminal shocks alone would cause an increase in the response. The results of Cason and Katcher (3a) show that there is no measurable natural effect of subliminal electrical stimulation on breathing or eyelid responses, and so no such control test was given in the present experiment, but, instead, a larger number of shock-response or *training combinations* was used than previously. Supraliminal shocks were used in the training combinations (*a*) At the beginning of the experiment before the limen was determined and (*b*) At intervals throughout the experiment, in order that the subliminal shocks might be very close to the limen. We attempted to follow the limen which often fluctuated a good deal by administering supraliminal shocks at various times, but approximately 80 percent of the shocks were subliminal.

Apparatus.—E and S were in different rooms which were 40 feet apart and separated by three doors and a wall. The apparatus in the two rooms was connected by a cable of wires. Telegraph relays served as signals from E to S and from S to E.

An electric light current of 110 volts and 60 cycles was the source of the electric shock. There was a 50,000 ohm fixed resistance and a 5,000 ohm variable resistance in the circuit, and also a rheostat from which the shunted current passed to the S's arm. The rheostat was equilibrated into 150 units.

The electrodes placed on the subject's arm were made of tin foil $1\frac{1}{2}'' \times 1\frac{1}{2}''$ which were soldered to the ends of the wires coming from E's room. Cheese cloth was sewed around the tin foil and around a long piece of felt cloth on the outer edge of the tinfoil. The tinfoil and cloths were moistened in .9 percent saline solution and the ends of the felt cloth were kept in the saline solution throughout the experiment serving as wicks to keep the electrodes in a constant state of moisture. Thin rubber bands were tied around cardboard strips on the arm to equalize pressure and to hold the electrodes in place. The electrodes were placed four inches apart on the left forearm which rested comfortably on a large board or table. S was instructed to keep his left arm relaxed and to refrain at all times from touching it with his right hand.

Procedure.—The procedure with regard to the shock was uniform for all three responses, and it may be described before the description of the procedure for each of the three responses separately. The procedure with regard to the shock was as follows:

1. The electrodes were fastened on S's arm.

2. S was told to press the signal key whenever he felt a shock no matter how weak the shock might seem to be. He was also asked to signal in all cases of doubt.

3. In the first few *training combinations*, supraliminal shocks were used and the successive shocks became weaker until they were subliminal. Then a series of sub-

liminal shocks one or two points on the rheostat below the felt shocks were used in the training combinations. Supraliminal shocks were given from time to time to make certain that the subliminal shocks were very close to the limen even though it fluctuated. The following is a typical procedure. Coincident with the signal for the increased response (breathing, winking, or foot-pressure), a shock of strength 50 on the rheostat would be given and kept on during the period (10 or 15 sec) of the increased response. If the shock was felt, a shock of strength 30 was given in the next training combination. If it was also felt, then in succeeding training combinations 25 was given and if not felt, 28 was given and if it also was felt 27 was tried. If 27 was not felt, it is a subliminal shock only one point on our scale below the limen. After ten *training combinations* using 27, 28 was given again and if it was felt, we knew that the limen was at the same point it had been before, and we could give several repetitions of shocks of strength 27 again. The *training combinations* were given at irregular intervals.

4. After 2½ hours of *training combinations*, subliminal shocks were given to determine their efficacy in eliciting an increased response. If, for instance, the shock was on for ten seconds in the *training combinations*, then in the test period the response was compared in alternate shock-on shock-off periods of 10 seconds each.

Consideration of Separate Responses

1. *Increase in Breathing*

Apparatus.—The device used to measure breathing was a belt apparatus[1] which fitted closely around the S's body in the region of the epigastrium. Whenever the total length of the belt changed, a mechanism in the belt made a series of electric contacts, so that the contacts were made whenever the circumference of the part of the S's body located just beneath the belt changed. The breathing apparatus was in circuit with a fast marker in E's room, and the stylus of this marker recorded the contacts made in the belt apparatus on the smoked paper of a long paper kymograph. A time record was made by a Jaquet graphic chronometer, and a third line on the smoked paper indicated when the subliminal shock was on and off.

Procedure.—At a signal from E, S voluntarily took two deep breaths thus greatly increasing the number of contacts in the belt apparatus. During the period the subject was taking the two deep breaths, a subliminal or supraliminal shock was on. Since for most subjects, taking two deep breaths required about 15 seconds, the shock was kept on 15 seconds uniformly in each training combination. After 150–600 training combinations, the number of contacts made on the belt apparatus during alternate 15 second periods of shock-on and shock-off was recorded on the kymograph paper.

Subjects.—Eight men, all students at the University of Wisconsin, were used in this procedure.

Results.—Breathing contacts as recorded on the smoked paper were counted in the alternate shock-on and shock-off periods; and the total contacts in the first ten shock-on shock-off pairs, in the second ten pairs, and the total contacts in all twenty pairs are shown in Table 1. The ratio of contacts

[1] This apparatus is described by H. and E. B. Cason, Affectivity in relation to breathing and gross bodily movement, *J. Gen. Psychol.*, 1933, **9**, 130–156.

made during the periods when the shock was on to those made when the shock was off is also shown. There is no sign of conditioning since the shock-on shock-off ratio is never significantly above one.

TABLE 1

NUMBER OF BREATHING CONTACTS IN ALTERNATE 15-SECOND PERIODS OF SHOCK ON AND SHOCK OFF (8 SUBJECTS)

Subject	Contacts in 1st 10 Pairs	Contacts in 2d 10 Pairs	Total Contacts	Ratio On-Off
A On	332	253	591	.97
Off	320	279	611	
B On	31	60	91	.72
Off	62	64	126	
C On	159	106	264	1.04
Off	143	109	252	
D On	59	90	149	.93
Off	99	61	160	
E On	229	309	532	1.11
Off	201	275	476	
F On	139	126	265	1.03
Off	138	113	258	
G On	94	66	160	.74
Off	114	102	216	
H On	194	176	370	1.05
Off	178	175	353	

2. *Increase in the Frequency of the Eyelid Response*

Apparatus.—We used an apparatus similar to that previously employed by Cason (3) in measuring the number of eyelid responses. One end of a thin aluminum lever was attached to the S's left eyelid just above the free margin by a piece of sticking plaster. The lever was supported just to the left of his left eye by a pair of 'spectacles' which could be fitted to individual S's and worn comfortably. A piece of platinum wire was attached to the outer end of the lever and when the S winked, this wire made an electric contact with another piece of platinum wire just above it. The fast marker which had been in circuit with the breathing apparatus was now in circuit with the eyelid apparatus and the number of winks was recorded on the smoked paper. The other two lines on the smoked paper were the same as in the breathing response.

Procedure.—At a telegraphic signal from E, S winked voluntarily as rapidly as possible. At the end of ten seconds another signal informed the S that he could resume his normal winking rate. While the subject was winking rapidly the shock was on. This training combination was repeated 150 times at irregular intervals for 2½ hours. At the end of this period the number of eyelid responses was recorded in alternate 10 second periods of shock-on and shock-off.

Subjects.—Twelve university students were used as subjects, and there were three men and nine women.

Results.—Table 2 shows the number of winks in the first, second, and third series of ten pairs of alternate shock-on and

TABLE 2

NUMBER OF WINKS IN ALTERNATE 10-SECOND PERIODS OF SHOCK ON AND SHOCK OFF (12 SUBJECTS)

Subject		Winks in 1st 10 Pairs	Winks in 2d 10 Pairs	Winks in 3d 10 Pairs	Total Winks	Ratio On-Off
A	On	50	26	48	124	1.51
	Off	22	24	36	82	
B	On	270	305	295	870	2.05
	Off	89	174	161	424	
C	On	161	138	171	470	1.63
	Off	96	85	107	288	
D	On	44	24	34	102	1.16
	Off	37	25	26	88	
E	On	43	25	20	88	1.04
	Off	38	24	23	85	
F	On	104	105	153	362	1.06
	Off	118	79	142	339	
G	On	128	117	103	348	1.00
	Off	123	127	96	346	
H	On	28	43	49	120	.90
	Off	42	39	52	133	
I	On	42	53	52	147	1.12
	Off	49	35	57	131	
J	On	36	66	70	172	1.04
	Off	35	58	71	164	
K	On	10	15	12	37	1.08
	Off	7	14	13	34	
L	On	20	23	27	70	.81
	Off	31	33	22	86	
Retest B (after 25 days)	On	45	42	44	131	1.02
	Off	43	42	43	128	
Reconditioning of B	On	374	285	311	970	2.39
	Off	134	115	157	406	
Retest of C (after 11 days)	On	196	239	203	538	1.17
	Off	138	138	182	458	
Reconditioning of C	On	266	205	209	680	1.25
	Off	218	158	165	541	

shock-off periods. We obtained conditioning in the first three subjects, the ratios of winks with the shock on to winks with the shock off being 1.51, 2.05 and 1.63. Two of these S's were retested 25 and 11 days later, and some trace of conditioning still remained in one S. After 150 training combinations, we obtained reconditioning in both of these subjects, the shock-on shock-off ratios being 2.39 and 1.25.

3. *Increase in Foot Pressure*

Apparatus.—A foot-pedal supported by heavy springs[2] was used to measure foot pressure. The pressure exerted on the foot pedal moved a pointer which indicated pounds pressure on a scale marked off into half-pound divisions. The working parts of the foot pedal apparatus and the scale were hidden from the S's view by a screen. Twenty pounds pressure would close an electric circuit, and a light would flash on in a box facing the S marked "You are not pressing hard enough," and a 35 pound pressure would close a circuit which would signal in the E's room and also cause a light to flash in a box in front of S marked "You are pressing too hard."

Procedure.—At a telegraphic signal from E, S pressed the pedal until the light appeared on in the 35-pound box, and he held this pressure until another signal informed him that he could release the pressure. After the strong pressure had been released, the pressure was kept just above 20 pounds. There was a change of approximately 15 pounds when the pressure was increased. The increased pressure was maintained for fifteen seconds and the shock was on during this period. After 100–150 of these train-

TABLE 3

TOTAL POUNDS PRESSURE AND POUNDS PRESSURE AT 5-, 10-, AND 15-SECOND READINGS IN ALTERNATE 15 SECOND SHOCK-ON SHOCK-OFF PERIODS (6 S's)

Subject		Sum of 5-sec rdgs.	Sum of 10-sec rdgs.	Sum of 15-sec rdgs.	Total Pounds Pressure	Ratio On-Off
A	On	803	794	795	2392	.99
	Off	798	800	798	2396	
B	On	379	379	377	1135	1.01
	Off	375	369	370	1124	
C	On	699	699	700	2098	.99
	Off	703	701	701	2105	
D	On	563	560	558	1681	1.01
	Off	550	551	551	1652	
E	On	542	543	545	1630	1.01
	Off	536	534	536	1606	
F	On	432	438	442	1312	.99
	Off	442	441	438	1321	

[2] This apparatus is described in more detail in a forthcoming paper by Edna Zartman in this JOURNAL.

ing combinations, readings of foot-pressure were made during alternate 15-sec periods of shock-on and shock-off, and three readings of pressure were taken within each of these 15 second periods. The experimenter administered the shocks by a noiseless switch and took readings of pressure in the same room with the S during this test period, but E was separated from S by a screen.

Subjects. Six university students were used as subjects, and there were three men and three women.

Results.—Neither the five-, 10-, or 15-second readings show any increase foot pressure during the shock-on periods, the ratio of pressure in shock-on periods to that in shock-off periods being either .99 or 1.01 in the case of all subjects.

Summary and Conclusion

The results clearly indicate that with the procedure and apparatus used, we were not able to condition an increase in breathing or an increase in foot pressure to a subliminal electrical shock, but that we were able to condition an increase in the frequency of the eyelid response to subliminal electrical stimulation in the case of three subjects. Our results were positive with three subjects, and reconditioning was obtained with the two of these who came back. Perhaps the reason for the positive results with winking is the simplicity of this response in comparison with the other two responses used.

An attempt was made to condition (*a*) increase in breathing, (*b*) increase in winking frequency, and (*c*) increase in foot pressure to subliminal stimulation. Eight subjects were used in breathing, and none of these were conditioned. Twelve subjects were used in winking, and three of them were conditioned and two reconditioned. Six subjects were used in foot pressure, and none of these were conditioned.

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STUDIES IN CROSS EDUCATION. IV. PERMANENCE OF TRANSFER

BY T. W. COOK

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The authors of two important recent works on the problems of learning interpret the results of earlier investigators of cross-education in a way that has important implications for a theory of transfer in general. Dunlap [1] cites the work of Norcross to illustrate his conclusion that the apparent transfer from one part of the body to another is illusory. Improvement in the efficiency of one limb from previous practice with another is due to general adjustment to the learning situation, and (or) to the presence of movements common to both practice and transfer trials. Wheeler and Perkins [2] consider that Bray's experiment presents adequate evidence that there is no true intra-organic transfer. The apparent transfer from one muscle group to another is in reality due to improved insight.

Although the writer began work on cross-education with a theoretical position quite similar to that of Wheeler and Perkins, his results are so directly opposed to those of Bray that further experimentation on problems not yet thoroughly worked out seems desirable. One such problem is the permanence or transience of the transfer.[3] Does the influence of previous practice show only in the first few transfer trials, or does it continue, throughout the whole transfer series, to add its effect to that of the additional training? This question is the subject of the present investigation.

The experiments described in our earlier papers on cross-education dealt with transfer from hand to foot and foot to hand, as well as from foot to foot and hand to hand. In the present instance it was thought best to study only hand to

[1] Habits, Their Making and Unmaking, Liveright, New York, 1932, 115–117.

[2] Principles of Mental Development, Crowell, New York, 1932, 322–323.

[3] Cook, T. W., Studies in Cross Education, this JOURNAL, 1933, 41, 698–699.

hand transfer. Besides economy of time, several factors contributed to this decision. Hand to hand transfer is the easiest of the four to investigate; in addition, the hands are more nearly equal in ability than are hand and foot—a factor of some importance for the method used to equate group differences; finally, transfer is greatest to the opposite and symmetrical muscle group, and, since the question at issue is the existence of a permanent residue from previous practice, it is advisable to look for such a residue where it is most likely to be found.

Four experiments were conducted. In Experiments *A* and *B* the subjects traced a star-shaped maze seen indirectly in a mirror. The construction and procedure in using the mirror-maze have been described elsewhere.[4] In the present investigation an improved form of stylus was used,[5] and slight

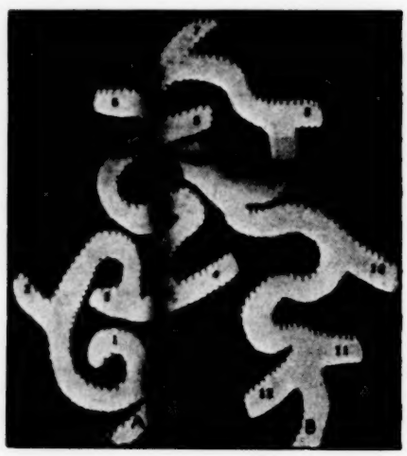

FIG. 1. Finger maze.

changes were made in procedure. Preliminary trials with direct view of the maze were omitted, and the common starting and end point of the true path was the 'crossroads' at the entrance to the two blind alleys farthest from the subject.

The apparatus for Experiments *C* and *D* was made by driving 1 in finish nails into the surface of a basswood board 20 × 20 in and 1 in thick. The nails projected 1/2 in perpindicularly to the surface of the board, and were about 1/4 in apart. The fact that the irregular contour formed by these nails was slightly unpleasant to rub against tended to lessen any disposition to follow the edge of the path in tracing. Although the

[4] Cook, T. W., Studies in Cross Education, this JOURNAL, 1933, 41, 145–147.

[5] The new stylus was made by Mr. S. H. Morrison.

maze path was made as irregular as possible, it is roughly horse-shoe shaped, beginning on the subject's left (Fig. 1, *A*) and ending on his right (Fig. 1, *B*). The length of the true path is 54 in, and its width about 1½ in. While tracing, the subject was blindfolded by means of a pair of leather goggles with cardboard discs pasted over the eyepieces.

In preparation for the experiment each subject was seated before the maze, which was fastened on a table at a suitable height, and asked to read the following typewritten instructions:

"You are to trace a maze with the tip of your forefinger while blindfolded.

"Both time and errors count: Learn to avoid the blind alleys and go as fast as you can.

"Keep your finger in contact with the floor of the maze. Your finger will drop over the edge when you reach the goal.

"The experimenter will say: Ready; GO; at the beginning of each trial.

"Do not start until the word: GO.

"Rest your hand comfortably between trials."

After the instructions were mastered the subject was requested to shut his eyes and trace a small practice maze; he then opened his eyes and looked at the pattern he had just followed. This initial practice was designed to minimize confusion in the actual experiment. While tracing the subject was allowed to touch the maze pattern only with his forefinger. Exploration with other fingers or parts of the hand was not permitted. Stop-watches or stop-clocks were used for timing. In mirror-tracing the interval between trials was 20 sec; in the first experiment (*C*) with the finger maze each trial was separated from the next by a 30 sec interval, in Experiment *D* the rest interval between trials was 15 sec. Each cul-de-sac error was indicated in the record by the corresponding number shown in Fig. 1, each retracing error by the symbol "O."

The method of equating right-left and group differences in ability was the same as that used in our earlier experiments on cross-education. One group of subjects was practiced with

Experiment	Group	No. of Practice Trials	Hand	No. of Transfer Trials	Hand
		Mirror-tracing			
A.....	I (20Ss)	10	right	10	left
A.....	II (20Ss)	10	left	10	right
B.....	III (10Ss)	30	right	30	left
B.....	IV (10Ss)	30	left	30	right
		Irregular Finger Maze			
C.....	V (12Ss)	10	right	10	left
C.....	VI (10Ss)	10	left	10	right
D.....	VII (20Ss)	30	right	30	left
D.....	VIII (20Ss)	30	left	30	right

the right hand and afterwards tested for transfer to the left hand; with a second group this order was reversed: the left hand had the practice and the right hand the transfer trials. Averaging the results for the two groups deleted group and left-right differences in ability. The arrangement of the four experiments is outlined below:

Only one session was required for each subject in Experiment *A* or Experiment *C*. In Experiment *B* and Experiment *D* 10 trials were taken at each daily session except the third, which consisted of 20 trials—10 with each hand. For example, the order of trials with Group III, Experiment *B*, was: 1st day, right hand, 10 trials; 2d day, right hand, 10 trials; 3d day, right hand, 10 trials, left hand, 10 trials; 4th day, left hand, 10 trials; 5th day, left hand, 10 trials. The subjects were students of Acadia University: women in Experiments *A* and B; men in Experiment *D* and (with four exceptions) in Experiment *C*. All were right-handed.[6]

Averages of the time records (mirror-tracing) and time and errors (finger maze) for each group appear in Table I and Table II. The data from all four experiments indicate that the transfer of learning from 10 or 30 practice trials persists through the corresponding 10 or 30 transfer trials. Since we are primarily interested in comparing the last trials of the practice series with the last trials of the transfer series, the graphs in Fig. 2 and Fig. 3 begin with trial 6. For Experiment *A* (broken lines, Fig. 2) and Experiment *D* (Fig. 3) the permanence of the transfer seems established. The differences between the last few practice and transfer trials are less than four times their P.E., but the consistency of the data, and the fact that in the last 4 of the 10 transfer trials of Experiment *A* and the last 17 of the 30 transfer trials of Experiment *D* the

[6] Students from the writer's class in the Psychology of Learning acted as observers in part of the experiments. Ten subjects in Group I and 10 subjects in Group II were under the direction of Miss Ruth Ingraham and Miss Evelyn Thurber. Experiment *B* was conducted by Miss Florence Jodrey and Miss Betty Williams, one half of Experiment *D* by Mr. G. L. McCutcheon and Mr. C. L. Stacey, and the other half by Mr. R. H. Lawerence and Mr. H. L. Stanfield. It is my pleasant duty to thank these for their faithful coöperation. It is significant that, although none of the observers were acquainted with the author's earlier work and thus had no expectation of any particular outcome, the same trends appear in all the experimental data.

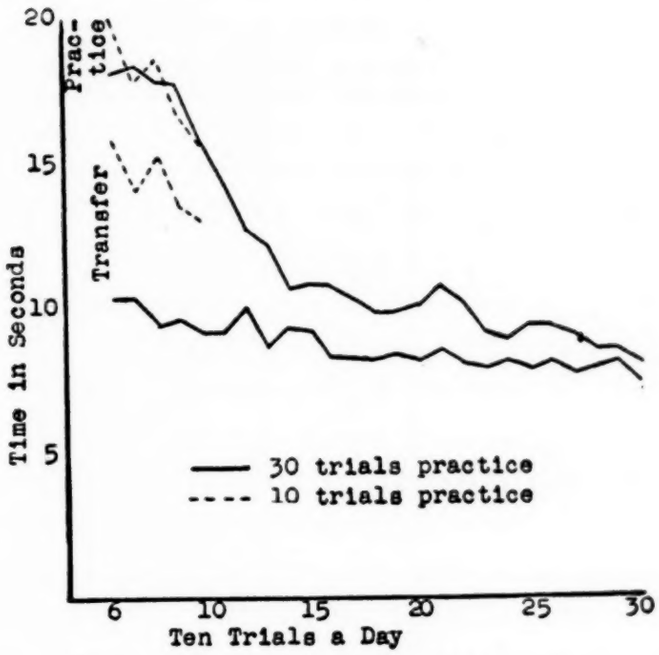

FIG. 2. Bilateral transfer in mirror-tracing.

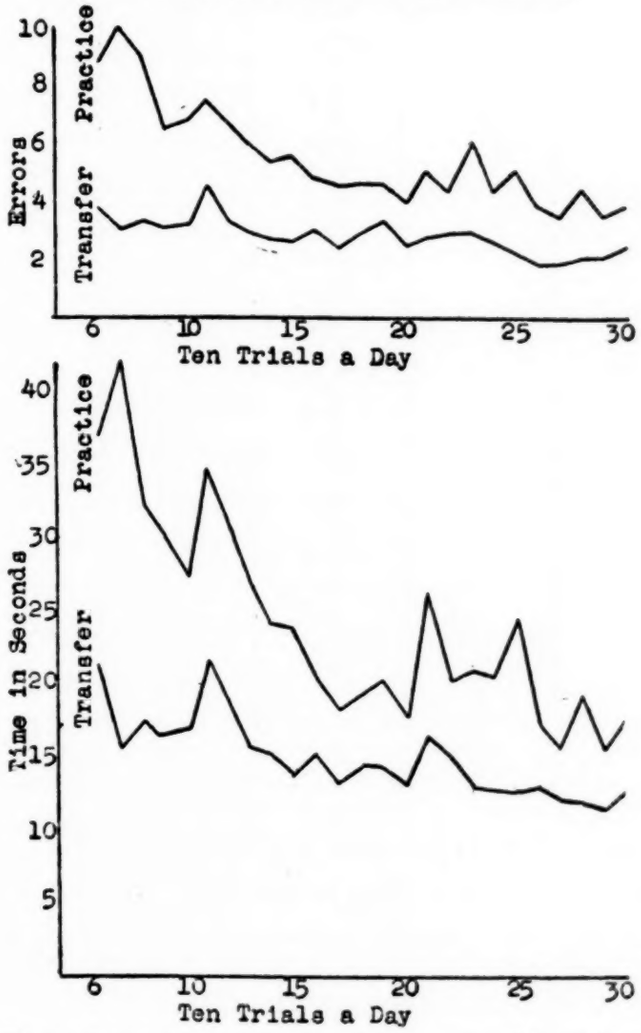

FIG. 3. Bilateral transfer in tracing a finger maze of irregular pattern.

TABLE I

PRACTICE AND TRANSFER TRIALS IN MIRROR-TRACING TIME IN SECONDS

Trials	Practice			Transfer		
	Group I R.H.	Group II L.H.	Av.	Group I L.H.	Group II R.H.	Av.
1	45.0	40.8	42.9	29.7	23.9	26.8
2	26.2	30.1	28.2	19.8	19.7	19.7
3	28.3	24.4	26.4	17.8	18.2	18.0
4	24.9	21.5	23.2	16.0	17.6	16.8
5	23.8	23.3	23.6	15.7	15.6	15.7
6	21.8	19.2	20.5	14.8	17.0	15.9
7	17.6	18.1	17.8	13.7	14.5	14.1
8	19.4	17.8	18.6	14.9	15.5	15.2
9	17.9	15.5	16.7	12.8	14.2	13.5
10	17.4	13.8	15.6	12.8	13.2	13.0
Trials	Group III R.H.	Group IV L.H.	Av.	Group III L.H.	Group IV R.H.	Av.
1	58.6	49.3	54.0	10.9	12.8	11.9
2	32.8	26.4	29.6	11.3	11.3	11.3
3	27.9	20.7	24.3	11.9	11.9	11.9
4	25.6	18.9	22.3	11.4	9.8	10.6
5	20.6	19.7	20.3	11.5	10.7	11.1
6	19.2	17.0	18.1	11.4	9.4	10.4
7	18.4	18.3	18.4	10.9	9.9	10.4
8	20.0	15.6	17.8	10.1	8.7	9.4
9	20.0	15.4	17.7	10.6	8.6	9.6
10	18.1	13.5	15.8	9.8	8.6	9.2
11	14.7	13.9	14.3	9.2	9.2	9.2
12	12.5	12.7	12.6	10.7	9.3	10.0
13	12.1	12.1	12.1	9.0	8.2	8.6
14	10.3	11.0	10.7	9.6	9.0	9.3
15	10.4	11.2	10.8	9.8	8.6	9.2
16	11.1	10.4	10.8	8.6	8.0	8.3
17	10.4	10.2	10.3	8.5	8.1	8.3
18	9.6	9.8	9.7	8.3	8.0	8.2
19	9.6	9.8	9.7	8.7	8.1	8.4
20	10.2	9.8	10.0	8.6	7.8	8.2
21	10.4	10.9	10.7	8.8	8.2	8.5
22	9.5	10.7	10.1	8.7	7.3	8.0
23	9.3	8.9	9.1	7.8	7.8	7.8
24	8.8	9.0	8.9	8.5	7.7	8.1
25	9.6	9.2	9.4	8.2	7.4	7.8
26	9.4	9.4	9.4	8.6	7.6	8.1
27	8.9	9.1	9.0	8.0	7.4	7.7
28	8.7	8.3	8.5	8.0	7.8	7.9
29	8.7	8.3	8.5	9.2	7.0	8.1
30	8.0	8.0	8.0	7.4	7.2	7.3

subjects made better time and fewer errors than in *any* of the practice trials,[7] increase the reliability of the findings to a

[7] The seeming exception of trial 21 in the time record for the finger maze is due to the 'warming-up' period which follows the 24 hr rest interval. This rise in the curve at the beginning of a day's trials appears in all our data.

considerable extent. The practice-transfer difference cannot be ascribed to group differences in ability, since the same subjects were used in practice and transfer trials.

In Experiment *B* only 20 subjects were tested (as compared with 40 in Experiment *A* and in Experiment *D*), and the small difference between the data for the last three practice and transfer trials (solid lines, Fig. 2) would have slight significance were it not in the same direction as that found in the other experiments. However, the amount of the superiority of transfer over practice records in Experiment *A* and Experiment *D* would lead us to suspect that in Experiment *B* chance factors have obscured the permanence of transfer to some extent, and that the true difference between practice and transfer speed is slightly greater than appears from the data. In Experiment *C* (10 practice and 10 transfer trials on the irregular finger maze) the advantage of the final transfer trials over the final practice trials is also small, but in this instance the qualitative results are of first importance. Observation of the behavior of the subjects showed clearly that high values of time and error records in some or all transfer trials with a given subject are no indication of the absence of transfer. Two factors enter here:

1. During the first few trials some subjects made radical changes in their method of tracing, with resulting loss of orientation and marked increase in time and errors. These changes of method occurred sometimes in the (10) practice trials, but were more frequent in the early transfer trials after 10 trials practice, probably because the novelty involved in the shift from one hand to the other made a certain reorientation necessary and thus encouraged exploration. A somewhat obvious example of this principle is found in the results of Subject *K*. As is well known, it is possible to get through a maze in a manner uniform from trial to trial, and with a minimum of exploration, by following one side of the path. In Experiment *D* subjects were told at the beginning of the experiment that following one side was an inefficient method, but in Experiment *C* no such instructions were given. Indeed, few subjects used this procedure, and of these all but one or

TABLE II

PRACTICE AND TRANSFER TRIALS IN LEARNING A FINGER MAZE BLINDFOLDED

Trials	Time in Seconds						Errors					
	Practice			Transfer			Practice			Transfer		
	Group V R.H.	Group VI L.H.	Av.	Group V L.H.	Group VI R.H.	Av.	Group V R.H.	Group VI L.H.	Av.	Group V L.H.	Group VI R.H.	Av.
1	400.0	204.8	302.4	98.3	43.9	71.1	74.7	44.0	59.3	27.3	9.9	18.6
2	85.0	134.5	109.7	40.0	34.3	37.1	18.6	42.0	30.3	7.4	8.3	7.8
3	67.5	125.0	96.2	34.7	34.4	34.5	18.9	28.6	23.7	7.3	8.4	7.8
4	56.0	71.9	64.0	32.6	70.4	51.5	11.9	21.8	16.8	6.8	19.3	13.0
5	43.2	71.0	57.1	43.8	26.1	35.0	9.2	20.2	14.7	10.1	6.0	8.0
6	68.0	47.6	57.8	32.3	26.9	29.6	14.3	13.8	14.0	5.4	6.2	5.8
7	44.2	30.6	37.4	40.2	29.5	34.8	10.2	7.2	8.7	7.2	6.1	6.6
8	37.5	47.0	42.2	44.1	35.3	39.7	8.5	12.7	10.6	7.0	8.6	7.8
9	32.6	26.1	29.3	33.5	29.1	31.1	7.3	5.8	6.5	5.7	6.2	5.9
10	47.4	32.4	39.9	38.7	30.3	34.5	15.7	8.6	12.1	6.7	7.7	7.2

TABLE II—*Continued*

Trials	Group VII R.H.	Group VIII L.H.	Av.	Group VII L.H.	Group VIII R.H.	Av.	Group VII R.H.	Group VIII L.H.	Av.	Group VII L.H.	Group VIII R.H.	Av.
1	272.5	169.1	220.8	27.1	34.5	30.8	46.8	28.6	37.7	4.9	5.0	4.9
2	130.8	61.3	96.0	26.6	30.7	28.6	25.8	12.4	19.1	6.0	6.0	6.0
3	65.0	54.5	59.7	22.2	28.2	25.2	17.8	10.1	13.9	5.5	5.1	5.3
4	47.0	42.9	45.0	19.0	22.5	20.7	11.8	8.5	10.1	3.6	5.4	4.5
5	63.1	41.7	52.4	16.5	22.7	19.6	17.2	8.5	12.8	2.7	4.5	3.6
6	40.5	33.6	37.0	18.9	23.7	21.3	10.0	7.8	8.9	3.3	4.5	3.9
7	45.3	38.8	42.0	15.0	16.5	15.7	11.0	9.1	10.0	2.6	3.7	3.1
8	34.8	29.8	32.3	16.0	19.3	17.6	10.9	7.1	9.0	3.0	3.8	3.4
9	31.6	28.5	30.0	14.7	18.4	16.5	7.1	6.0	6.5	2.6	3.8	3.2
10	30.0	25.0	27.5	14.7	19.3	17.0	6.5	7.1	6.8	2.6	4.0	3.3
11	32.7	37.0	34.8	18.8	24.7	21.7	6.2	8.9	7.5	3.6	5.7	4.6
12	29.7	31.8	30.8	18.2	19.8	19.0	6.2	7.3	6.7	2.6	4.3	3.4
13	24.2	30.3	27.2	13.6	18.0	15.8	5.3	6.8	6.0	2.4	3.5	3.0
14	26.9	21.3	24.1	13.4	17.1	15.2	5.8	5.0	5.4	2.4	3.0	2.7
15	22.6	25.0	23.8	12.4	15.5	13.9	5.6	5.6	5.6	2.5	2.7	2.6
16	20.4	20.8	20.6	13.5	17.0	15.2	5.2	4.6	4.9	2.4	3.8	3.1
17	18.7	18.1	18.4	12.0	14.6	13.3	4.6	4.5	4.6	1.8	3.0	2.4
18	18.5	20.1	19.3	12.8	16.5	14.6	3.8	5.6	4.7	2.4	3.6	3.0
19	18.4	22.1	20.2	12.5	16.2	14.3	4.0	5.4	4.7	3.3	3.5	3.4
20	16.1	19.8	18.0	11.8	14.4	13.1	3.5	4.5	4.0	2.4	2.7	2.5
21	21.9	30.5	26.2	17.0	16.8	16.9	4.5	5.7	5.1	2.3	3.3	2.8
22	19.5	21.0	20.2	13.5	16.6	15.0	3.8	5.0	4.4	2.6	3.4	3.0
23	19.6	22.5	21.0	12.7	13.2	13.0	7.5	4.9	6.1	2.4	3.5	3.0
24	20.7	20.7	20.7	13.4	12.4	12.9	3.4	5.4	4.4	2.5	2.8	2.6
25	29.4	19.8	24.6	12.5	13.0	12.7	6.0	4.2	5.1	2.0	2.6	2.3
26	14.7	19.7	17.2	14.4	11.7	13.0	2.7	5.0	3.9	2.0	2.1	2.0
27	13.8	18.0	15.9	11.1	13.1	12.1	2.4	4.7	3.5	1.3	2.6	2.0
28	20.7	17.8	19.2	11.8	12.2	12.0	5.0	4.1	4.5	1.8	2.6	2.2
29	15.1	16.3	15.7	11.2	12.0	11.6	3.3	3.8	3.6	2.1	2.3	2.2
30	18.3	17.6	17.9	13.4	12.4	12.9	3.4	4.3	4.8	2.2	2.8	2.5

two soon realized that the immediate gain was overbalanced by a lack of economy in later trials. Subject *K*, however, followed one side or the other in all practice trials except the sixth. Throughout the first 5 transfer trials he followed the same side as he had in the last 4 practice trials; then on the sixth transfer trial he changed his method with startling consequences. The number of errors for practice trials 1 to 10, right hand, were: 7, 8, 6, 13, 10, 48, 6, 6, 6, 6. The novel errors in trials 4 and 5 were retracing errors; in trial 6 *K* left the side and lost his bearings. For the transfer trials with the left hand there were 7, 6, 6, 6, 5, 9, 17, 28, 11, 28, errors in trials 1 to 10, respectively. The cul-de-sac errors for the last 4 practice trials and the first 4 transfer trials were always nos. 1, 3, 4, 5, 9, 12 (See Fig. 1. The extra error on the first transfer trial was a retracing error). *K* was keeping close to the inner side of the path. On the fifth transfer trial, however, he discovered that blind alley no. 12 could be avoided by cutting across its entrance to the end of the true path. This gave him a taste for adventure, and on the next attempt he left the shelter of the side and went boldly out into the centre, with disastrous effect on his time and error record. Yet examination of *K*'s results shows clear evidence of permanence of transfer. From the nature of the case this could not appear in his error record, but in the first transfer trial he made better time than in the last trials of the practice series, and the gain in speed is continuous thereafter until the débâcle in the sixth transfer trial. Here the time for the individual trials is significant: practice, right hand, 150, 87, 81, 133, 98, 343, 119, 106, 84, 84 sec; transfer, left hand, 78, 70, 74, 63, 55, 106, 177, 257, 138, 212 sec.

2. The results from the work of Subject *H*[8] illustrate a principle that became increasingly evident during the experiments. Exploration of the whole pattern is essential to complete mastery of the maze. If this exploration does not take place in early trials, subsequent entrance into a novel cul-de-sac may disorient and confuse the subject to a marked degree. The number of errors trial by trial for Subject *H* were

[8] Not included in the group average because *H* was left-handed.

as follows: right hand, 9, 8, 5, 5, 5, 3, 5, 2, 3, 70; left hand, 16, 10, 58, 66, 41, 54, 151, 84, 18, 31. The paucity of errors in the first 9 practice trials was not a consequence of *H* following the side of the path. *H* was merely 'lucky' in the first trial. He entered only a few blind alleys, and (probably because of this) found it easy to recall the true path. But on practice trial 10 a 'chance' variation of his pattern of movement brought him into blind no. 6. He then became bewildered, and this bewilderment was carried over to the transfer trials.

It seemed evident to the experimenter that under these circumstances there could be little hope of securing reliable quantitative results on transfer from 10 trials practice with the irregular finger maze. The disturbing factors (change of method, etc.) might occur as often in practice as in transfer trials, and their influence could thus be 'averaged out' if enough subjects were tested, but the variability in individual transfer records after 10 trials practice (with the finger maze) is so great that at least 100 subjects would be required.

On the other hand, if we except the rise at the beginning of each day's work, Fig. 3 shows only slight fluctuations from (transfer) trial to trial after 30 trials practice, and observation of the subjects' behavior and study of the time and error data for each reagent show that instances of sudden and striking loss of orientation are almost completely absent from the transfer records after 30 practice trials. These facts would lead us to suspect that a certain stability of pattern is requisite for the maximum amount of positive transfer. This does not mean that there is no transfer unless the pattern is stabilized during the practice series. The evidence seems rather to indicate that the transfer, as measured by comparative efficiency in practice and transfer trials, may be positive or negative. A change of method or a loss of orientation during the last few (of 10) practice trials may transfer to the other hand and mask the positive transfer.

The above conclusion so far rests chiefly on analysis of individual performance after 10 and 30 trials practice with the finger maze. But another approach is possible. If our deductions are sound, there should be a higher *percentage* of

transfer (*i.e.*, ratio of amount transferred to amount learned) after 30 trials practice than after 10 trials practice. Table II and Fig. 2 indicate clearly that the *absolute* amount of transfer from 30 trials is much greater than from 10 trials—a result that coincides with the findings of the author's previous work on cross-education. Examination of the evidence that the ratio of amount transferred to amount learned increases with the number of trials practice, involves a comparison of data from the present and earlier experiments. This comparison will be made in a later paper.

Summary

Transfer from hand to hand after 10 and 30 trials practice in mirror-tracing a star-shaped maze, and after 30 trials practice with an irregular finger maze (Ss. blindfolded) was still effective at the end of 10 and 30 transfer trials, respectively.

Transfer from 10 trials practice with the finger maze, while showing some evidence of permanence of transfer, was so variable that no quantitative deductions could be made. Individual records indicated both positive and negative transfer. It is concluded that positive transfer is not maximal until the learned pattern had attained a certain degree of stability.

(Manuscript received March 19, 1934)

THE COMPARATIVE MEMORY VALUES OF PLEASANT, UNPLEASANT AND INDIFFERENT EXPERIENCES *

BY ROD MENZIES

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In this experiment we attempted to secure measures of memory in relation to feeling-tone. Since our aim was to throw some light on ordinary, normal activities, we used pleasant, unpleasant and indifferent experiences of everyday life as memory materials and avoided unnatural laboratory situations as much as possible.

Several studies, more or less similar to this one, have already been made, but since some of these studies showed greater memory value for pleasant experiences than for unpleasant, and other studies did not show this at all, there seemed to be need for a further study of recall in relation to affectivity. Our experimental methods were not very different than those employed in some previous studies save that we made more comprehensive and objective measures of feeling-tone and amount of recall than have been generally made heretofore. The aim was to measure the following four aspects of the recall of verbal material as related to quality and intensity of feeling-tone: (1) amount of recall as related to feeling-tone present during original experience, (2) amount of recall as related to feeling-tone present during recall, (3) frequency of revival as related to feeling-tone, and (4) changes of feeling-tone at successive reproductions.

The results of this study have a bearing upon psychological hedonism—a theory too familiar in the history of psychology to need exposition here. Shop-worn as it is, hedonism persists as a perennially attractive hypothesis,

* I wish to acknowledge my indebtedness to Professor Hulsey Cason of the University of Wisconsin for directing my attention to this problem, and for many valuable suggestions during experimentation and the preparation of this paper.

because were its truth established, psychology would be provided with a simple, fundamental law which would serve as a wide net in which to gather up a thousand and one psychological jetsam. In recent times a number of distinguished psychologists have sponsored hedonism in one form or another. Among these, L. T. Troland (**12**) has worked out a systematic psychology with 'retroflex action,' a pleasure-pain mechanism, as a basic principle of human motivation; E. L. Thorndike (**11**, 176–177) has urged that satisfyingness and annoyingness are conditions of learning and unlearning; Sigmund Freud (**2**) and his followers have emphasized the fundamental importance of the 'pleasure principle' in all human activity. In this study we are particularly interested in the psychoanalytic proposition that unpleasant memories are repressed into the unconscious (**4**).

Previous Studies

Many of the experiments dealing with memory as related to feeling-tone have involved the use of more or less artificial laboratory materials, such as odors, colored figures, lists of words, etc. In the following summary, for the sake of brevity, we shall omit studies of such a nature, since they do not bear so directly upon our problem as do those experiments in which normal, everyday memory processes were used. We have selected from the rather extensive literature on this subject only those studies which seem most pertinent to this particular investigation. Those who are interested in a more extensive survey of the work done in this field should consult Meltzer's summary (**7**) and Cason's very thorough monograph (**1**). For convenience we shall classify experiments into two groups: (**1**) those experiments which appear to support the proposition that pleasant experiences are more completely recalled than are unpleasant ones; (**2**) those experiments which show no significant differences in recall of pleasant and unpleasant experiences.

Of the first group we shall review five studies. Meltzer (**8**) investigated the recall of 132 college students for events of a Christmas vacation. Of pleasant experiences listed, 42.60

per cent were forgotten after six weeks, while of the unpleasant experiences, 59.73 per cent were forgotten. Thomson (**10**) measured, among other materials, the recall of experiences which had been recorded in diaries. She used 30 subjects for one experiment and 100 for another, all high school students. Pleasant experiences were recalled better than unpleasant ones. Koch (**5**) studied memory for school grades as related to feelings aroused by the grades, with 76 college students as subjects. Five weeks elapsed between receiving the grades and the measured recall. Pleasant grades were best recalled, but unpleasant grades were better recalled than indifferent ones. Jersild (**3**) had 51 college students briefly record the pleasant and unpleasant experiences of the past three weeks, and 21 days later write a second recall of the same materials. In the second recall, an average of 45.69 percent of pleasant items and 31.67 percent of unpleasant items of the first recall appeared again. In Stagner's (**9**) experiment, 103 college students each wrote a brief description of the most pleasant experience and also of the most unpleasant experience of the past 15 days and listed the associations provoked by each experience. Three weeks later copies of these descriptions were returned to the subjects, with the lists of associated items omitted, and the subjects were asked to list all the associations they could then remember. The average 'redintegration' of pleasant associations was 54.4 percent of the first recall, and of unpleasant associations was 43.4 percent of the first recall.

Among experiments of our second group (*i.e.*, those which show no significant differences in the recall of pleasant experiences as compared with unpleasant ones) three are of particular interest in this connection. For Kowalewski's (**6**) experiment of 1908, one of the earliest on this problem, 105 school boys wrote brief descriptions of the pleasant and unpleasant events of the preceding day, a holiday. Ten days later they wrote second recalls of the same events. More pleasant material than unpleasant appeared in these recalls, but at the second recall a slightly smaller percentage of the pleasant experiences was remembered than of the unpleasant. Wohlgemuth (**14**) used 687 children as subjects and employed

essentially the same methods as Kowalewski. The period between first and second recalls was 10 days for one group and 14 days for another. He found that 40.1 percent of originally pleasant materials of the first recall were forgotten in the second recall, and 39.8 percent of the unpleasant materials were forgotten. Cason (1, section 4, p. 30–38) in one of his studies of memory as related to feeling-tone used 90 college students as subjects, and had them recall events of the previous week and grade them for intensity of feelings as they remembered to prevail at original occurrence and as they found now at recall. Also an estimate was made of completeness and accuracy of memory for each event. Three weeks later another recall was made and the subjects again graded their feelings at recall and judged completeness and accuracy of memory. The originally unpleasant and originally pleasant incidents were remembered equally well, but those incidents accompanied by the more intense feelings, whether pleasant or unpleasant, were somewhat more efficiently recalled than those accompanied by feelings of low intensity or by indifference. Substantially the same relationships were found to hold between feelings prevailing at recall and extent of recall. The results of the study we are about to report point to these same general conclusions.

Procedure

Fifty students, 26 women and 24 men, at the University of Wisconsin, nearly all members of a class in abnormal psychology, volunteered to act as subjects. Ages ranged from eighteen to twenty-five years. Briefly the method was as follows: each subject wrote a description of some experiences of the preceding day, a week following this he wrote a second description of the same experiences, and then made another recall three weeks after the second. Thus, the recalls occurred one day, eight days, and 29 days after the original experiences. Also each subject judged the intensities of feeling-tones which had prevailed during the original experiences and which prevailed at recalls. At the second and third recalls, the subject estimated the frequency of revival of each incident since the previous, written reproduction. Subjects were not told the purpose of the experiment, and were not forewarned that they would be asked to give second and third recalls.

The subject was given printed instructions to read before each recall. These were designed to describe the routine of the experiment in detail and to impress the importance of careful and thorough work. For the first recall the subject was asked to write a description of some experiences of yesterday. There were no rules regarding the nature of these incidents, the subject was simply asked to give experiences he could

'readily and clearly recall,' and no matter whether they were pleasant, unpleasant or indifferent. The importance of describing every detail, no matter how trivial it might seem, was stressed. On the other hand, adding details from imagination was discouraged.

The subject was also asked to estimate the intensity of pleasantness or unpleasantness associated with these incidents. He rated the feeling-tones which he judged had prevailed during the experiences of the previous day and also the feeling-tones present then at recall, making an estimate each time his feelings changed appreciably. The rating scale used had a middle point 'O' for indifference or complete lack of feelings, five plus steps for equal increments of pleasantness, and five minus steps for equal increments of unpleasantness. Plus 5 indicated the most intense pleasantness the subect was capable of feeling, and minus 5 the extreme of unpleasantness.

At the second and third recalls essentially the same procedure was followed, the incidents of the first recall were described in writing again, and the feelings present at recall were rated. Subjects were impressed with the importance of describing every detail, no matter how trivial. Each subject was generally alone in one of the laboratory rooms while he wrote, although occasionally two or three persons worked in the same room. Care was taken to obviate distractions and hasty work. The incidents described were the run-of-the-mine of everyday college life, and dealt with classes and examinations, parties, trips, love affairs, etc.

The writer, who was the experimenter on all occasions, carefully divided this material into logical units in approximately the manner described by Whipple (**13**, 208), taking particular pains to apply the measure as consistently as possible. For the sake of convenience in handling the data, logical units rated + 5 and + 4 were classified as 'intensely pleasant,' those rated + 3 and + 2 were classified as 'moderately pleasant,' and those rated + 1 as 'slightly pleasant.' Unpleasant units were classified in the same manner. The material for all subjects and for the three recalls totaled 6,060 logical units.

Results

Results were calculated by totaling the items of all subjects, rather than by calculating results for each individual and then taking averages. This method of ours did not provide data on individual differences. For a number of reasons, however, it seemed to be the proper method to employ in this case. In computing percentages recalled, the first recall was taken as 100, not because it was supposed to represent 100 percent recall, but simply for the sake of convenience in making comparisons.

Ideas appearing in the first recall were considered to be reproduced in the second or third recalls when the same meaning was repeated, regardless of changes in the form of expression. Ideas appearing in the second or third recalls but not in the first were disregarded. These, however, were few in number.

TABLE I

AMOUNTS RECALLED AS RELATED TO FEELING-TONES OF ORIGINAL EXPERIENCES

(Logical units of the first, second and third recalls when classified according to feeling-tones of original experiences. The second and third recalls are also given in terms of percentage of the first recall.)

Feeling-Tones	First Recall Logical Units	Second Recall		Third Recall	
		Logical Units	% of First Recall	Logical Units	% of First Recall
Intensely Pleasant	250	170	68.0	176	70.4
Moderately Pleasant	850	575	67.6	467	54.9
Slightly Pleasant	217	116	53.5	107	49.3
Total Pleasant	1317	861	65.4	750	56.9
Indifferent	192	129	67.2	104	54.2
Slightly Unpleasant	291	164	56.4	158	54.3
Moderately Unpleasant	693	463	66.8	440	63.5
Intensely Unpleasant	205	142	69.3	151	73.7
Total Unpleasant	1189	769	64.7	749	63.0
Grand Total	2698	1759	65.2	1603	59.4

Table I shows the extent of recall expressed as logical units and as percentages of the first recall with the units classified according to feeling-tones attending the original experiences. 'Intensely pleasant' here, for example, means intensely pleasant during first occurrence, and without reference to the feeling-tone which might attend recall. At the second recall 65.4 percent of pleasant units were revived; 67.2 percent of the indifferent; and 64.7 percent of the unpleasant. At the third recall the results were 56.9 percent of the pleasant units revived; 54.2 percent of the indifferent; and 63 percent of the unpleasant. We found, then, no significant relation between *quality* of feeling-tone of an experience and the extent of its recall. Fig. I shows, however, that extent of recall tended to vary directly with the *intensity* of feeling-tone. There was a deviation from this tendency in the case of indifferent items of the second recall, but in the third recall the relation was fairly consistent. This tendency appeared to assert itself most clearly, then, in the case of long time retention.

Table II shows the extent of recall expressed as logical units and as percentages of the first recall, with the units

classified according to feeling-tones attending recall, rather than attending first experience, as in the case of Table I. It should be noted that the figures in the 'First Recall' column of Table II were not taken as 100 in computing percentages.

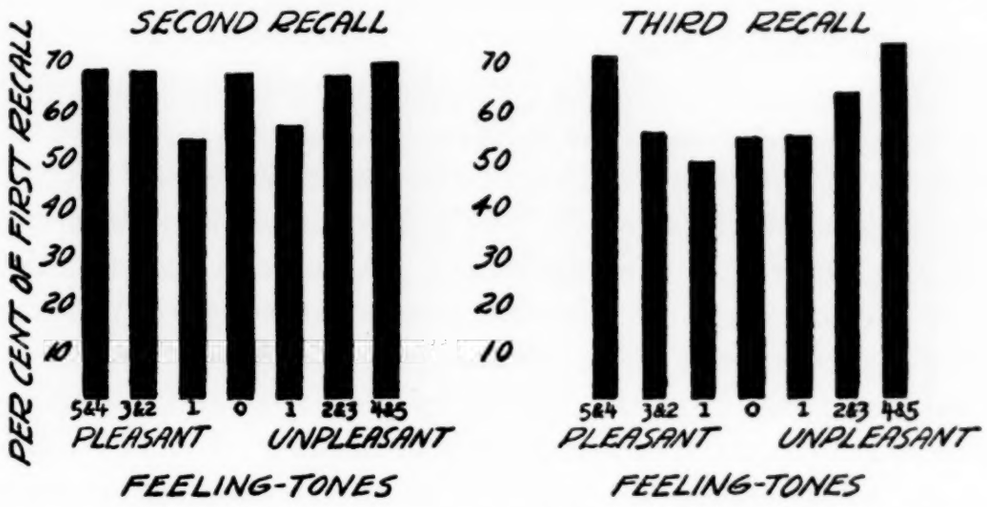

FIG. 1. Percentages recalled as related to feeling-tones of original experiences.

TABLE II

AMOUNTS RECALLED AS RELATED TO FEELING-TONES ATTENDING RECALLS

(Logical units of the first, second and third recalls, when classified according to feeling-tones present at the time of recall. The second and third recalls are also given in terms of percentage of *equivalent* material of the first recall. The amounts of this equivalent material are not identical with the figures shown under First Recall, since many items of a given feeling grade at first recall shifted to other feeling grades at subsequent recalls. The figures which served in the calculation of percentages are not shown in this table.)

Feeling-Tones	First Recall Logical Units	Second Recall		Third Recall	
		Logical Units	% of First Recall	Logical Units	% of First Recall
Intensely Pleasant	44	7	70.0	14	73.7
Moderately Pleasant	682	288	67.0	218	66.9
Slightly Pleasant	398	349	73.0	355	63.2
Total Pleasant	1124	644	70.2	587	64.7
Indifferent	884	794	73.7	703	65.5
Slightly Unpleasant	396	192	76.5	163	74.1
Moderately Unpleasant	245	120	78.4	136	76.4
Intensely Unpleasant	49	9	112.5	14	82.4
Total Unpleasant	690	321	77.9	313	75.4
Grand Total	2698	1759	73.1	1603	66.9

For example, at the second recall 7 units were intensely pleasant, and since 10 units describing the same incidents had appeared in the first recall, the percentage recalled was 70. The incidents given in the other 34 units of the intensely pleasant of the first recall appeared in the second recall under some other feeling grade.

At the second recall 70.2 percent of the pleasant units were revived; 73.7 percent of the indifferent; and 77.9 percent of the unpleasant. At the third recall the results were 64.7 percent of the pleasant units recalled; 65.5 percent of the indifferent; and 75.4 percent of the unpleasant. Memories accompanied by unpleasant feelings showed somewhat superior memory value as compared with pleasant or indifferent ones. It should be noted that the 'intensely pleasant' and 'intensely unpleasant' items here are too few in number to yield reliable results with respect to these cases of extreme feeling-tone. The third recall (Figure II) shows some evidence of the same

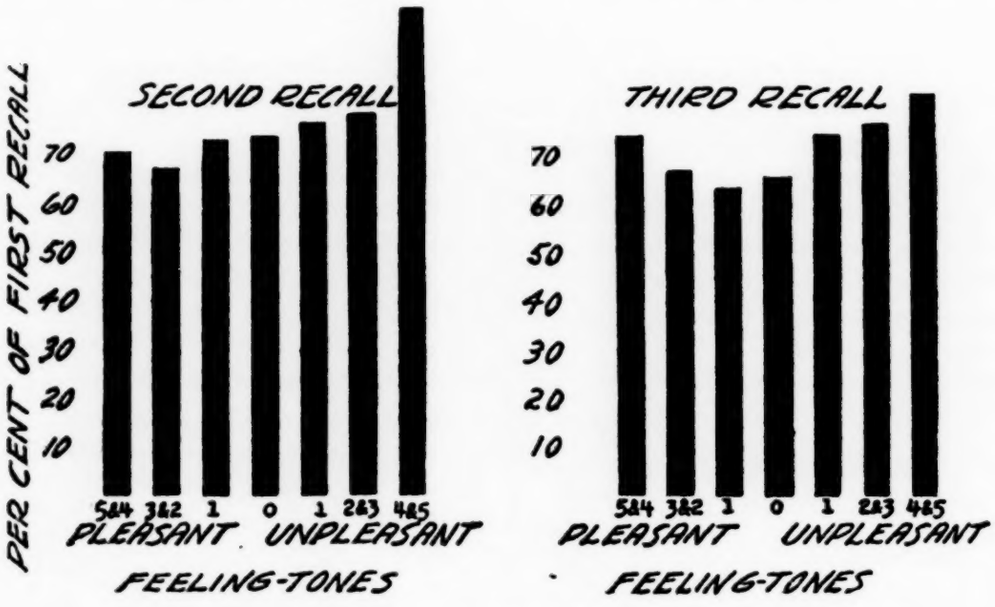

FIG. 2. Percentages recalled as related to feeling-tones attending recalls.

tendency, noted above, for extent of recall to vary directly with the *intensity* of feeling-tone, but there is little or no evidence of such a tendency in the second recall.

When he wrote the second and third recalls, each subject estimated the number of times he had thought about these incidents since the previous writing. Such an estimate was

made each time a feeling-grade was assigned, and so we were able to compare frequency of revival with feeling-tone. These data are given in Table III. Feeling-tone here refers

TABLE III

FREQUENCIES OF REVIVAL OF EXPERIENCES AS RELATED TO FEELING-TONES ATTENDING ORIGINAL EXPERIENCES

(Average frequencies of revival of experiences, exclusive of written reproductions, when classified according to feeling-tones of original experiences.)

Feeling-Tones	Between 1st and 2d Written Recalls—7 Days	Between 2d and 3d Written Recalls—21 Days	Between 1st and 3d Written Recalls—28 Days
Intensely Pleasant	3.3	2.7	6.0
Moderately Pleasant	3.0	1.8	4.8
Slightly Pleasant	1.1	1.4	2.5
All Pleasant	2.8	1.9	4.7
Indifferent	2.7	1.0	3.7
Slightly Unpleasant	2.4	0.6	3.0
Moderately Unpleasant	2.6	0.9	3.5
Intensely Unpleasant	4.1	2.1	6.2
All Unpleasant	2.9	1.1	4.0

to that of original experience. The average frequencies of revival, exclusive of the written recalls, for the period between first and third recalls (28 days) were: 4.7 times for pleasant events; 3.7 for indifferent; and 4.0 for unpleasant. The differences are too small to be considered significant. Frequency of revival does not seem to have depended upon *quality* of feeling-tone, but Table III shows that frequency of revival increased with *intensity* of feeling-tone in a fairly regular fashion.

Table IV shows the average values of feeling-grades at successive recalls. For example, those incidents graded as +5 (intensely pleasant) at original occurrence had depreciated to an average value of +3.8 at the first recall, after one day; to +1.6 at the second recall, after 8 days, etc. Events recalled one day after occurrence had faded very decidedly in affective strength (more so for the unpleasant than the pleasant); and seven days later, at second recall, they had depreciated still further in feeling value; but after the next twenty-one days, at third recall, but little change had occurred. This phenomenon

TABLE IV

INTENSITIES OF FEELING-TONE AT SUCCESSIVE RECALLS

(Average values of feeling-tone at first, second and third recalls when classified according to the values assigned to the original experiences. For example, items graded as + 5 at original occurrence were graded, on the average, as + 3.8 at the first recall, etc.)

Original Experience		First Recall 1 Day After 1st Experience	Second Recall 8 Days After 1st Experience	Third Recall 29 Days After 1st Experience
Extremely Pleasant	+5	+3.8	+1.6	+1.4
	+4	+2.5	+1.5	+1.5
	+3	+1.0	+0.9	+0.9
	+2	+1.1	+1.1	+0.7
	+1	+0.5	+0.4	+0.3
Indifferent	0	+0.1	+0.3	+0.1
	−1	−0.2	−0.1	−0.3
	−2	−0.5	0.0	−0.2
	−3	−0.7	−0.7	−0.4
	−4	−2.4	−0.9	−1.2
Extremely Unpleasant	−5	−2.5	−1.8	−1.0

of initially high rate of loss which diminishes to approach zero has its analogue in the forgetting of some kinds of verbal materials.

DISCUSSION

Retention of these experiences was doubtless increased by writing them, and by repeated recalls. Under ordinary conditions these same materials would be more rapidly forgotten. Also, the second and third reproductions may in some cases have been simply repetitions of the previously written descriptions rather than revivals of the original experiences. An effort was made to counteract such a tendency by asking the subject to re-live his original experiences as completely and vividly as possible while he wrote his description.

We do not suppose that a given feeling grade represented exactly the same intensity of feeling in the case of every subject. Individuals differ in affective capacities and opportunities, and a given degree of pleasantness, for example, might to one person be 'intense' and to another be only 'moderate.'

Our measure of frequency of revival was not absolutely reliable. The subject's estimate of the number of times he had recalled a given memory could not be checked, and in some cases was probably incorrect.

These inaccuracies we have mentioned, however, would be expected to prevail with about equal weight in the case of pleasant, unpleasant and indifferent experiences. This being so, measures used for comparative purposes would not be invalidated.

It is worth noting that these memories were drawn from a single day of each subject's experience and involved rather mild feelings, generally speaking. A special selection of very intense incidents might show quite different results, particularly in the case of unpleasant experiences of traumatic nature.

Our attempt was to measure remembering with the subject exerting a maximum of effort and attention in order to recall every detail of his experiences. The results seem to show that under such conditions pleasant and unpleasant memories have about equal value. But these results show how remembering *can* operate, not necessarily how it *does* operate. It is quite possible that in everyday circumstances when there is no special obligation to achieve a complete reproduction, there is a tendency with many persons to slur over unpleasant incidents, or neglect them altogether. In many of the previous experiments on this problem, no attempt was made to exhaust the subject's memory, but rather a number of experiences were simply listed as rapidly as possible. If a tendency to neglect the unpleasant in superficial recall does exist, it might operate with considerable force in such experiments, and might account, in part at least, for the superior memory value for pleasant incidents which a number of these studies have shown.

The results of this investigation are opposed to Thorndike's law of effect, Troland's retroflex principle, and similar learning mechanisms. There is no evidence here of repression of unpleasant memories such as the psycho-analysts claim to operate. It is of course possible, putting it in psycho-analytic terms, that the censor operated to prevent complexes subject to repression from appearing in these memories.

In his monograph on this subject, Cason (1, p. 71–91) has synthesized and summarized the results of his own and previous investigations in a number of 'General Conclusions

and Propositions.' The results of this present study seem to support the following four of his conclusions:

"6. There is little, if any, difference in the efficiency with which activities that were *P* and *U* when they originally occurred can be retained and reproduced." (*P* means pleasant, and *U* unpleasant.)

"11. The efficiency of retention and reproduction is greater when the affective factor was present during the original learning."

"12. There is little, if any, difference in the efficiency of reproducing activities and experiences that are P and U when they are reproduced."

"13. The efficiency of reproducing past activities and experiences is greater when the affective factor is present on reproducing these activities and experiences."

Summary

Amounts of recall for pleasant, unpleasant and indifferent experiences were measured one day, eight days and 29 days after occurrence, and measures were also taken of intensities of feeling-tones. The following results were found.

1. Frequencies of revival of pleasant, unpleasant, and indifferent experiences showed no important differences. But these frequencies increased as intensities of feeling-tone increased, and this relationship was significantly consistent.

2. There were no significant differences in the percentages recalled of pleasant, unpleasant and indifferent experiences. This was true whether feeling-tones of first experiences or of recalls were considered. These percentages of recall, however, increased as intensities of feeling-tone increased, and although this relationship was not always consistent, it does appear to be a significant tendency.

Thus, frequency and extent of recall for ordinary life experiences appear to be correlated with *intensity* of feeling-tone rather than *quality* of feeling-tone.

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A BLIND PUZZLE-BOX

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A blind puzzle-box of an unusual type has for some time been used in the Psychological Laboratory of the University of Denver. This puzzle is intended to demonstrate a simple learning situation in the case of human subjects and to represent a transition from animal learning with puzzle-boxes to more complicated sensory-motor learning in human subjects. However, it could easily be used in its more unbreakable form, the perforated brass type, for the study of learning in the case of primates. The wooden and aluminum forms are used for human subjects. Perhaps one of bakelite or pyraline is to be recommended for all round laboratory service.

Figure 1 *A* and *B* gives some idea of the appearance of the

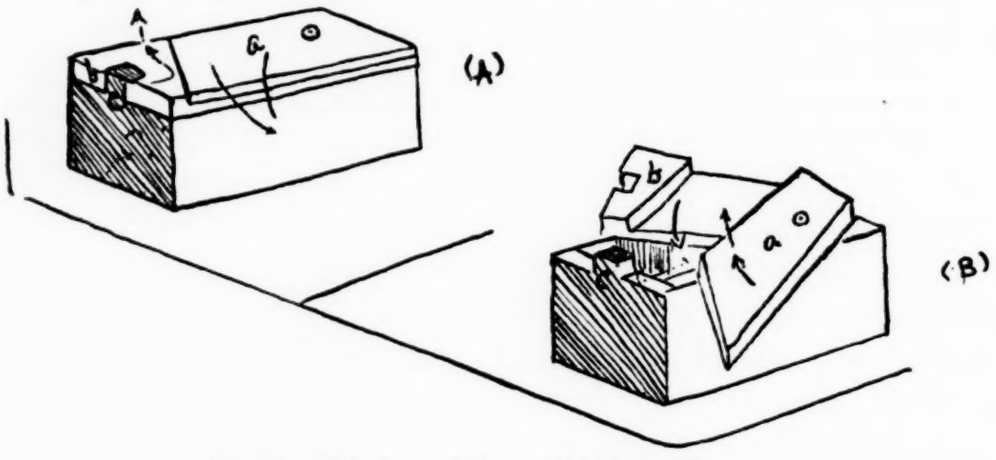

FIG. 1. Blind puzzle box—(*A*) shut, (*B*) open.

box, which has three parts: (*a*) the box proper; (*b*) a false lid; and (*c*) the true lid. A screw fastens the two lids on to the box proper. (*b*), the false lid, contains near the screw, a mortice into which will fall a washer when the box is held upside down. (*c*) also contains a mortice and a slit at the screw end, but a cleat at the other end which holds it fast to the box proper (*a*) when the washer is in place in the mortice of (*c*). But the lid

will slide away from the cleat when the washer is dropped into the mortice in (*b*), and thus the box may be opened by sliding back the new lid, (*c*), on the screw and turning it either to the right or the left. All the while, the washer which permits the sliding back of lid (*c*) is concealed and this gives the puzzle-box its blind character.

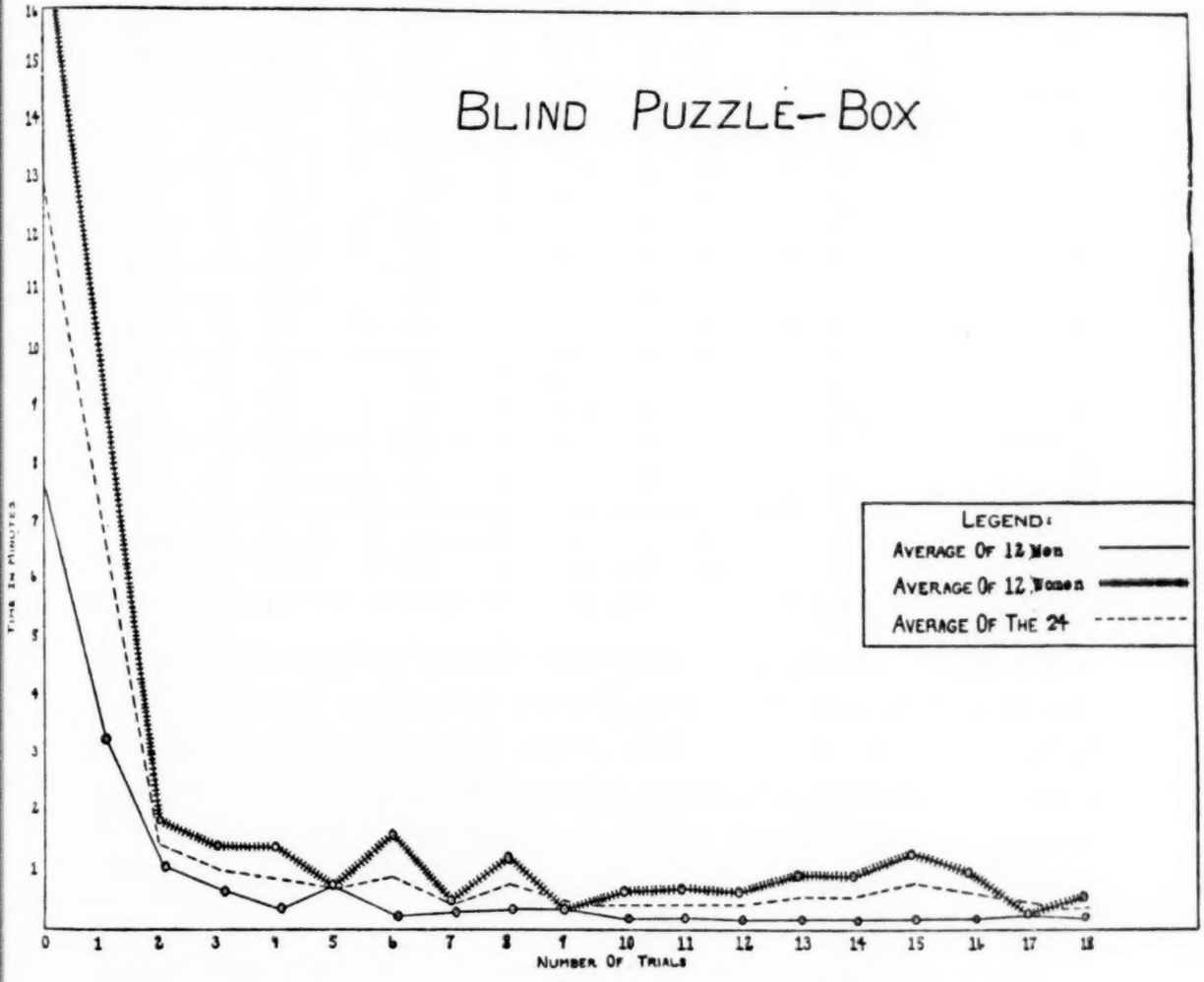

FIG. 2.

In administering the experiment, the experimenter should first demonstrate that the box can be opened by previously opening the box out of sight and presenting it thus opened to the subject. But in the laboratory situation, the puzzle-box is always presented to the subject closed and right side up. Upon placing the box in the hands of the subject with the request to open the box, time is taken with a stop watch. This starts off 'trial one' which is completed with the opening

TABLE I
THE BLIND PUZZLE BOX EXPERIMENT
Time is expressed in minutes.

No. of Trials	Men		Women		Total	
	Av.	S.D.	Av.	S.D.	Av.	S.D.
1	7.53	1.49	18.27	3.69	12.90	2.92
2	3.25	.71	9.59	3.03	6.42	1.94
3	1.10	.24	1.90	.54	1.50	.36
4	.65	.16	1.44	.36	1.04	.25
5	.34	.06	1.42	.39	.88	.24
6	.76	.26	.72	.19	.74	.23
7	.17	.04	1.67	.56	.92	.28
8	.23	.06	.47	.13	.35	.06
9	.29	.07	1.21	.40	.75	.25
10	.33	.09	.35	.06	.29	.08
11	.10	.02	.61	.13	.36	.10
12	.11	.02	.63	.15	.37	.08
13	.08	.02	.61	.16	.35	.10
14	.07	.01	.91	.30	.49	.17
15	.05	.01	.88	.29	.47	.16
16	.08	.02	1.29	.41	.69	.25
17	.08	.02	.94	.43	.51	.18
18	.12	.03	.22	.05	.17	.04
19	.08	.02	.53	.16	.30	.06
Totals	15.33	3.30	43.66	11.34	29.50	7.82
Average	.81	.18	2.30	.59	1.55	.41

of the box. Rarely does the inexperienced subject see through the situation, and he is non-plussed as to how he opened the box. Usually, it takes five trials, some of them with extremely large time measures, before the alert subject gets some insight as to the proper way to hold the box. Quite often, the subject finds himself solving the puzzle with less time, but little insight as to how it is done.

We present herewith the group curve (and data from which it was derived) of twenty-four students in Experimental Psychology—twelve men and twelve women; also curves for the men and the women and data. The curves indicate that the men learn the puzzle more readily than the women. There are more fluctuations in the curve of the latter. Note particularly, in their curve, Trials 7, 9 and 16. The men settled down sooner than the women, and an almost dead level of performance set in for them at the 11th trial.

Table II will show for eleven men and nine women the

TABLE II

COMPARATIVE LEARNING BY NUMBER OF TRIALS TO REDUCE THE TIME TO .02 MIN.

Men		Women	
Subject	No. Trials	Subject	No. Trials
1	26	1	19
2	24	2	22
3	28	3	32
4	102	4	46
5	41	5	29
6	82	6	65
7	17	7	77
8	27	8	86
9	47	9	161
10	38		
11	8		
Totals 11	440	9	537
Average	40		59.66

TABLE III

RELIABILITY COEFFICIENTS FOR LEARNING CURVE IN SOLVING BLIND PUZZLE-BOX

Total Curve			
γ	.81	1st Six Trials (I)	γ=.57
P.E.	.05		P.E. .11
		2d " " (II)	γ=.92
			P.E. .03
		3d " " (III)	γ=.95
			P.E. .01
		4th " " (IV)	γ=.98
			P.E. .01
		Total of II, III, & IV,	γ=.96
			P.E. .01

number of trials required to reduce the time to .02 of a minute. Here again the men seem to have the better of it for they require on the average 40 trials for reducing the time to a minimum, whereas, the women require an average of 59.7 trials for the same.

The reliability of the blind puzzle-box was determined by using the time curves of twenty students with the split halves method. (See Table II.) In this way a relaibility coefficient of .69 was obtained, but when we derived from that an index of reliability, we got .83, which gives a fair degree of reliability for the puzzle box.

It was desirable to know at what stage of the game the puzzle-box was most reliable. This was ascertained by breaking the curves into four parts, I, II, III, IV, *i.e.*, of six

trials each, and correlating the total odd and total even scores of these sections. It will be seen upon examining Table II that at first the blind puzzle-box is not very reliable for γ is only .57, but it becomes more satisfactorily reliable in the second part and remains so for the other sections of the curve. In fact, if we take the total scores for the last three sections of the curve and determine their reliability, it is found to be .95. From these results, and the handling of the same, we may offer the blind puzzle-box as an apparatus for laboratory exercises. A further application of the puzzle in a study of learning in some of the higher animals, as raccoons and primates, offers itself. However, the experimenter so far has been unable to find an opportunity of making such use of it.

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